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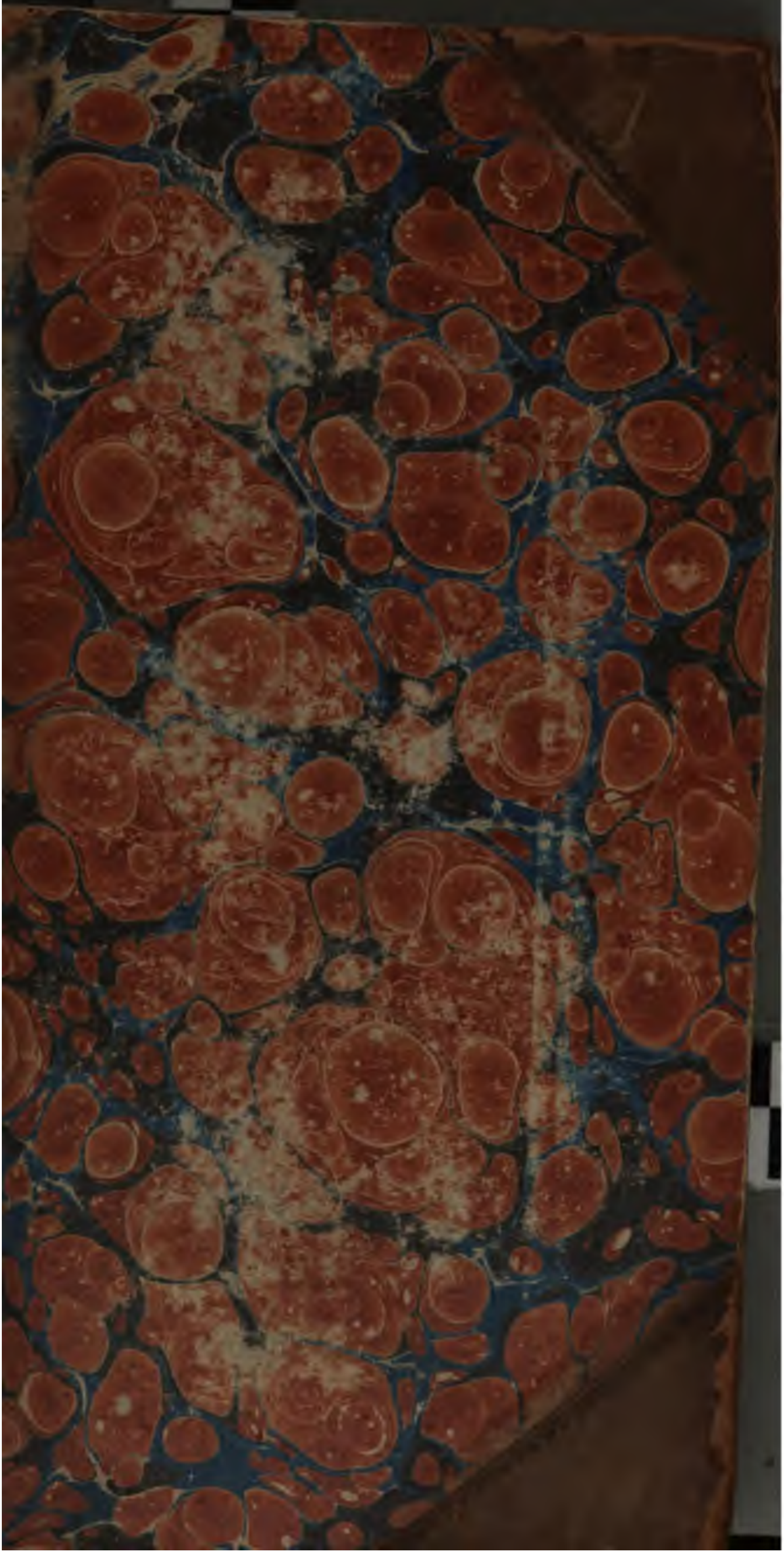

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OBSERVATIONS

ON THE

HEALTHY AND DISEASED PROPERTIES

OF

THE BLOOD.

BY

WILLIAM STEVENS, M.D.

"For the life of the flesh is in the blood."—Leviticus, xvii. 21.

LONDON:
JOHN MURRAY, ALBEMARLE STREET.
MDCCCXXXII.

721.

LONDON:
Printed by WILLIAM CLOWES,
Stamford-street.

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TO

HIS MAJESTY

FREDERICK THE SIXTH,

KING OF DENMARK,

&c. &c. &c.

SIRE,

SHOULD the following work be found to possess any value, it will, in some measure, be entitled to the protection of your Majesty; inasmuch as the investigation which it embraces was commenced in a distant colony subject to your sway. Or should the practice to which it may ultimately lead, tend to alleviate the sufferings, or lessen the mortality of malignant diseases, I know no one to whom this circumstance will be likely to give more sincere pleasure than the August Personage to whom I have now the honour of addressing myself. The heartfelt interest which You are known to feel in whatever concerns the welfare of the human race, has secured respect and esteem to your Majesty in every civilized country; while the anxious and paternal care with which you have invariably watched over, and attended to, the comfort of even the poorest individuals in the ancient realm over which you immediately reign, has fortified the throne of Denmark with the strongest of all bulwarks—in the grateful attachment and sincere prayers of a whole kingdom, for the personal happiness and prosperity of their Sovereign.

If the descriptions which I have given of the West-India fevers be true to nature, or if the theories which I have adopted be founded on truth, they will be sure to last; if not, they will soon fall, and even the kindest wishes of a wise Monarch will not change the current in their favour. It is not, therefore, with the hope of ensuring success that I have, with permission, dedicated this work to your Majesty: but having resided for many years in your West-India islands, I have felt, in common with thousands, the not unfrequent effects of your Majesty's benevolence; and gratitude for the many proofs which I have personally received of your kindness, induces me thus publicly to express what I feel, and, though the native of another country, and sincerely attached to the land of my birth, yet, in personal devotion to your Majesty, no native subject of Denmark can be more truly sincere, than the humble individual who has the honour to subscribe himself,

Your Majesty's
Most grateful and attached Servant,
WILLIAM STEVENS, M.D.

London, March 21st, 1832.

PREFACE.

THE writer of this Treatise had some years ago written a memoir on the fevers of the West Indies; but a variety of unforeseen circumstances have, hitherto, prevented him from being able to lay that work before the public. The observations on the healthy and diseased properties of the blood, contained in the present volume, have been transcribed, almost entirely, from the pages of that manuscript, and now published in a separate form, with the belief that every new fact relative to the vital fluid may be of importance, particularly at this moment, when not only the secluded cities of the Continent, but even the isolated shores of the British empire are visited by a new and a foreign pestilence, 'whose effect holds such an enmity with the blood of man*.'

Whether the epidemic cholera, which is now in Europe, be or be not, in the beginning, merely the effect of a nervous impression, may be afterwards considered; but almost the whole of those who have seen this most malignant disease are now agreed, that the blood, even at an early period, is deranged to such a degree as to be almost incapable of performing its functions. And, if so, it is evident that every additional fact relative to the healthy properties of the blood will be another step towards enabling us to understand the nature of those changes in this fluid which are, as I believe, not only in cholera, but in all the essential fevers, the chief cause of the diseased action which occurs in the solids, and very frequently the sole cause of death. Now, if this be true, (and I shall endeavour to assist my readers to come at the truth,) it is then probable that a more correct knowledge of the nature of those sudden changes may enable us to adopt a practice which will be less a matter of mere chance than it is at present; for as yet we have no compass to guide us in our treatment, except the effect of remedies, most of which are empirically administered, and even the lesson is lost which might be learned from the use of those agents which are in reality the most successful, from the fact that the result of these is at this moment in direct opposition to almost all the acknowledged doctrines of the day.

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* Hamlet.

In an inanimate machine, the quality of the fluid which acts as the propelling power may be of little consequence; but this is not the case in a living body, where the solids are affected fully as much by the quality as they are by the mere quantity of that mysterious fluid which stimulates them into action, irrigates every fibre, and is, in truth, the very source from which the whole of the organized structures are immediately derived. There are, also, some most important facts, which would lead us to believe, that 'the life of the flesh is in the blood;' or, in other words, that the solids derive their vitality from the same fountain to which they are indebted for their nutrition, and, as I believe, for their very existence.

The modern solidist, who himself looks only to a part of the body, boasts of the extension of his own ideas, and talks with contempt of the contracted views of the humoral pathologist; but my conviction is, that the notions of both are equally inconclusive: for, it is this narrow and unphilosophical system of looking merely to a part, that has done so much injury to the cause of science, and impeded, for a time, the diffusion of truth. It is this, also, that has so long retarded all improvement in the doctrine of fever; it is this which has given origin to doctrines so diametrically opposite, and yet so pertinaciously maintained by their different advocates, as to leave the profession of medicine completely open to the attacks of the scoffer; but, what is worse, it is this that has lessened its *utility* in practice, and prevented our science from attaining that high rank and respect, to which, in its real nature, it is justly entitled.

It is now, I believe, generally admitted, that the blood is endowed with vitality; and, if this be granted, it then follows, that this fluid, like everything else that possesses life, must be subject to disease; and I firmly believe that such is the fact: for the circulating current is not only an essential part of the living body, but, of all the constituents of the animal frame; it is also by far the most delicate, and so easily deranged, that even by the mere circumstance of its stagnation for a few seconds, in its own vessels, its properties are changed. It is likewise more easily affected by poisons, heat, cold, &c. than any of the solids: and if the blood, as in the malignant fevers, can be deranged to such a degree as to produce the disease, and frequently to be the cause of death, it is thence evident that we can only form a correct pathology, when we include the diseases of the fluids as well as of the solids. It is, moreover, my firm conviction that we can only adopt a successful treatment, when we attend not merely to a part, but to the whole system; for the vital fluid is the pabulum of the solids, but in fever it is always diseased, and, to use the language of the best observer of nature that perhaps ever lived, it often, as in cholera, 'turns to an infected jelly,' and those who neglect to attend to this can never be generally successful in their practice. It is often essentially necessary to attend to the solids, but in many malignant diseases we can only strike at the root of the evil by remedying the vitiated state of the vital fluid, the healthy condition of which is as essential to the healthy action of the various organs, as the purity of the air which we breathe is essential to the maintenance of life.

The science of medicine is greatly indebted to Dr. Prout, for having so ably drawn the attention of the profession to the great advantages which may be derived from the study of animal chemistry: and if he has dwelt but little on the nervous system, this is the less to be regretted; particularly at a period, when so much has been lost by the almost exclusive attention which has been devoted to the solids. There is, however, at this moment a more active spirit abroad; and many individuals in the field, of first-rate talents. Even the students are looking at present with becoming suspicion on the exclusive theories of Cullen or Broussais, and have now no longer the same veneration for ‘the magic of a name which they had but a few years ago; consequently, those doctrines are not likely to be long in favour, which are supported neither by fair experiments nor by the result of practice. Under such circumstances, may we not hope that a new era is about to dawn on the practice of medicine, and that common sense will replace those exclusive dogmas which have been so long the pride of physicians and the disgrace of their science? It is only, however, in the present day that animal chemistry has assumed a sober aspect or been usefully applied in the treatment of disease; and surely, from the facts which we now know, we may be allowed to reason on the effects which will naturally be produced by energetic agents which are known to enter the vital current; although we may no longer use the language of our predecessors, and talk about fermentation, concoction, the four humours, error loci, the stimulus of death, or think of oxygenating the blood in fever, cholera, &c. by means of acids, and other agents that are supposed to contain oxygen in a separable form.

That the blood is both black and diseased in the last stage of all those fevers that are speedily fatal, is now just as certain as that the sun is the great source of light. It is also more than probable that the physician, who is acquainted with the true cause of the redness of the vital stream in its healthy state and of its darkness in disease, will be more likely to restore this fluid to its original colour, when it becomes black, or its healthy properties, when it becomes deranged, than the mere solidist who adopts a philosophy that is only applicable to rigid tubes; whilst, both in his theory and practice, he forgets that there is either blood or any other fluid in the living body; and dreams from first to last of nothing but 'sympathy,' 'the nervous system,' or increased action in the 'internal surfaces of the mucous membranes.'

During a residence of nearly twenty years in the West India islands, I had but too many opportunities of observing the rapid progress, and the sudden deaths produced by the destructive fevers of hot climates. I had read much of what had been published on these diseases; but, up to a late period, a dense cloud overhung the whole, through which it was difficult to get even a glimpse at truth. I had, indeed, long been aware, that the different West India fevers were not mere varieties of the same disease, and that one of them, at least, did not correspond with the common descriptions which have generally been given of that fever, by most of our writers; and, as to our practice, so long as we attended merely to the solids, God knows, we were anything but successful: for, even the oldest and most experienced physician had, too frequently, the mortification of seeing his unfortunate patients bound and struggling in the iron grasp of a malignant disease, not only with an internal feeling on the part of the practitioner that he could do no good, but even with a thorough conviction that the remedies in common use were worse than none.

Mercury, at one period, was our chief agent in the treatment of fever: but, in mild cases, the remedy was more injurious than the disease itself; and, in bad cases, we had daily proofs that it could not cure the disease. Calomel may sometimes be used as a purgative in the first stage. There are also other diseases in which it may be used with great advantage; but mercury, in any form, is not an antidote to the febrile poisons. I have known individuals attacked with a malignant fever, even when they were in a state of salivation; and though their systems were charged with this mineral, still it had no effect, either in neutralising the cause or arresting the progress of the disease: for in such cases the fever ran its course with as much, and, in some instances, even more rapidity than in those in which not one particle of any mercurial preparation had been administered. This was the case with the mercurial preparations, and such was also the fact with many of the other remedies in common use.

Most of the older physicians in the West Indies still continue to regard the doctrine of Cullen with a degree of fondness which amounts almost to veneration; while many of the younger practitioners whom I have met with are as firm devotees to the doctrine of Broussais, and any attempt to lessen the importance of opinions deeply rooted, will naturally be likely to meet with a strong resistance. But be this as it may, I shall still have one satisfaction in the purity of my intentions, for utility is the only object which I have had in view; and if the conclusions be correct which I have drawn from the facts that I have stated, they will stand the test of time;

if not, my theories, like many others, will be but evanescent. Yet, even then, the practice which I have recommended will be found to be useful, were it for no other reason than this, *that it is more mild, and infinitely less pernicious*, than the present bold, but unscientific, practice, which is still but too commonly used in the treatment of fever in the Western world.

My attention was first strongly attracted to the present inquiry, whilst witnessing the rapid progress of some fatal cases of a malignant fever, which commenced in the island of St. Thomas during the hot months of 1827. Some facts which I then observed, together with former recollections, induced me to alter my opinion with respect to the nature of fever, and to adopt a method of treatment essentially different from any other that I had previously used. This treatment has now been fairly tried. The result gave origin to the manuscript to which I have referred, for this was written, partly with a view of recommending to others those remedies which I had found to be so successful in my own practice. I have also some hopes of doing good by drawing attention (particularly of the younger members of the profession) to the diseased state of the blood; a subject too much neglected by their predecessors, and of the greatest importance to the safety of those patients that may be committed to their care; for the error of looking merely to the nervous system, or some particular part of the body, as the sole cause of fever, has been, and, as I believe, still is the chief cause of the fatal errors which exist in the treatment of malignant diseases, not only within the tropics, but all over the world.

I do not mean, however, to underrate the importance of the nervous system, or to condemn the propriety of attending in our practice to the state of the solids; but I do say, that those pathologists take but a narrow and unphilosophical view of the animal economy who reason on disease as if there were not one drop of blood in the whole system, and forget that there is anything else in the living body except nerves, muscles, or mucous membranes. But fever, at least such as is met with within the tropics, is evidently not the effect of a nervous impression; neither are those fevers produced by local inflammation, either in the brain or the stomach: for often the nervous system is not affected until after the attack; and in certain fevers of a most malignant character there is no crust on the blood; whilst in these, as in cholera, in place of inflammation, there is, in the first stage, a want of circulation—not merely in a part, but in the whole system—which is directly the reverse of inflammatory action. But every drop of the blood is deranged even before the attack, and after this, every fibre is affected, whilst the brain and all the organs are under the influence *not always of organic, but invariably of functional disease.* Neither can we cure these fevers either by a nervous impression, or by the mere reduction of increased action: for we are only successful when we attend to the diseased state of the vital current, as well as to the solids, and use, at a proper period, those active remedies that produce their effects not merely on a part, but first on the blood itself, and then on all the solids in the system, through the medium of this their nutritive fluid.

Those who have seen the most of the pestilential fevers, know best how exceedingly malignant they sometimes are; and no one who values his own reputation will pretend to say that he can cure, by any practice, every patient whom he may be called upon to attend; for in some cases, the remote cause is applied in such a concentrated form, that the vital principle scarcely makes an effort to preserve itself. In such cases there is no re-action, and the patients, even under the most judicious treatment, sometimes die early in the disease;—in other cases, where the stomach, the liver, or some of the other important organs have been injured by previous disease, it occurs but too frequently, when there is great re-action, that the whole force of the fever appears to concentrate itself in the diseased organ; and then the efforts even of the best practitioners are frequently unsuccessful. The exceptions, however, which I have stated, are, comparatively speaking, extremely rare; and 'all diseases that soonest destroy the frame are most easily cured when we have once found out the true method of treating them*.' Two, at least, of the West India fevers run their course with great rapidity, and require the most active measures to arrest their progress; yet these, like the Indian cholera, however fatal they may have been, and still are, under the treatment of the mere solidist, still my conviction is that, even in their most malignant form, both their violence and mortality may be greatly lessened by the early and judicious use of proper remedies.

Dr. Jackson, who has written largely on the West India fevers, was, like most of the writers of his day, a complete solidist. He believed that fever was a nervous disease, and, like others who have adopted the same belief, he was most deplorably unsuccessful in his treatment; for such is the close connexion between theory and practice, that, good or bad, they invariably keep pace with each other; but, after having seen the effects of their several remedies, he never, perhaps, came nearer to truth, in all his writings, than when he said that 'the species of yellow fever which I have now described is universally acknowledged to be a terrible disease; and

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[*] Moseley on Tropical Diseases.

there are few, I believe, so uncandid as to boast of general success in the manner of curing it. *A road is therefore left open, not only for improvement, but almost for total innovation*[*].' That a road is left open, not merely for improvement, but almost for *total innovation,* is most religiously true; but whether or not I have approached nearer to this path than most of my predecessors, must be left, in some measure, to the judgment of my readers, but, above all, to the result of experience—for this is decidedly the best and, perhaps, the only real test of the true value of a medical doctrine.

The manuscript from which the greater part of the following work has been taken was originally written under circumstances certainly not favourable to literary pursuits. I may also state, that the number of cases of cholera which I have attended within the last two months has not only retarded the present publication, but prevented me from correcting the press with that accuracy which is so necessary in a work on science; but whatever charity I may require for the manner, I trust that there are some facts contained in the following pages that will be found deserving the attention of those who are not so firmly wedded to their own opinions as to believe that everything is erroneous which does not exactly correspond with their own preconceived views.

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[*] Jackson's *Treatise on the Fevers of Jamaica, &c.* p. 174.

Those who are engaged in active practice in the West India islands know but little of the improvements which are made, almost daily, on this side of the Atlantic; and since my return to England I have scarcely had sufficient time to consult the writings of others. It is, therefore, very possible, that I may have omitted to give credit to some individuals which is justly their due; and, if so, I shall endeavour to be the very first to correct the error: from these, therefore, I have nothing to fear. There are also many of my opinions which I may probably change; but in matters of fact I have stated nothing except what I believe to be strictly true, and truth, whatever may assail it, will ultimately maintain its course; and those who attempt to impede its progress, though they may succeed for a time, yet they will find it at last as hopeless a task as it would be to prevent the mountain torrent from finding its way to the ocean, into which it is ultimately to be received.

GENERAL OBSERVATIONS ON THE BLOOD.

In man, and all those animals that are most perfect, the blood during life never rests, for it is constantly in active motion, and runs in a double circle from the time that they first breathe, up to the last moment of existence. It is rendered impure in the extreme circulation, after this it is purified in the lungs, and then the pure blood is sent by the arteries all over the body, where a small part of it becomes solid; a part is removed by the secreting organs, and the rest is converted from arterial to venous in the minute vessels of the extreme texture all over the system where the arterial circulation ends and the venous begins. This impure blood is conveyed by the veins to the right side of the heart, and from this it is sent to the lungs, where it is again purified; chiefly by the immediate removal of that principle which had been the cause of its impurity in the venous current, and when this is effected, a given proportion of pure air is instantly attracted into the circulation. This purified or arterial blood passes on from the lungs through the pulmonary veins to the left side of the heart: by this it is propelled into the Aorta;

from which it is again distributed by the smaller arteries all over the living body, to supply the solids with their nourishment, and to perform other functions which are equally important in the animal economy.

Such is the course of the vital current, the discovery of which gave immortality to Harvey, and enabled him to leave to posterity the richest legacy that has yet been given to the medical profession.

The blood itself is the red saline fluid which circulates in the heart and vessels of all animated beings, supplying *materiel* for the formation of the body; and from this is derived the different fluids, which are almost constantly secreted in the various organs. Some of these are thrown out of the system, for, if retained, they would be not only useless, but even detrimental; while others are retained in the body, and serve purposes that are highly important.

In many of those creatures that are less perfectly formed, the fluid contained in the smaller vessels is almost colourless; but in all those animals that have a double circulation and a high temperature, the blood has a rich scarlet or *arterial* colour in the arteries. It is purple in the veins, and after receiving the addition of the impure blood of the Vena Portæ it is black, or almost so, when it arrives at the right side of the heart. But when this impure blood is exposed to the air in the pulmonary organs, it is instantly changed from purple or Modena red to a rich scarlet colour, and this it retains until the pure blood arrives in the extreme texture, where it again changes from arterial to venous.

When the blood is subjected to the process of the destructive analysis, the ultimate constituents of the grosser ingredients, the albumen, the fibrin, &c., are found to consist, like other animal matter, of carbon, oxygen, hydrogen, and azote. The following are the proportions as ascertained by Gay-Lussac, and Thenard.

Carbon	52.883
Oxygen	23.872
Hydrogen	7.540
Nitrogen	15.705
	100.000

But in addition to these, the residuum is found to contain soda, potass, lime, iron, &c., all of which had existed in the blood, not in their alkaline or metallic state, but combined with acids in the form of salts.

When the blood is first drawn from the body, it appears to be homogeneous in its consistence. This, however, is merely in appearance; for the vital current is in reality not only a compound, but a very complicated fluid; consisting of water, albumen, fibrin, colouring matter, a variety of saline ingredients, a minute portion of iron, a small quantity of an oily substance, and while in the circulation both the arterial and the venous blood contain a proportion of gas, which they hold, as I believe, not in solution but in a free state.

The following is the latest analysis of healthy blood that has been made in this country; it is that of Dr. Clanny:—

Water	756
Albumen, coagulated	121
Colouring matter	59
Free carbon	32
Fibrin pressed and dried	18
Muriates of soda and potassa, carbonate of soda, and animal extractive	14
	1000

The above analysis, however, differs materially from that which is given by the same author in a former work; but particularly in the addition of free carbon, which is probably an error.

The following is the most recent and, perhaps, the most correct analysis of the blood that has been made on the continent:—

STANDARD ANALYSIS OF THE BLOOD. By Mr. L. R. LECANU.
[Journ. de Pharmacie, No. IX. September 1831, p. 502.]

Water		780.145
Fibrin		2.100
Albumen		65.090
Fatty matter:	a, crystalline	2.439
	b, oily	1.310
Colouring matter		133.000
Extractive *soluble in alcohol water*		1.790
Albuminate of soda		1.265
Muriate of soda Muriate of potassa Carbonate, Phosphate, Sulphate } Alkaline		8.370
Carbonate of lime Carbonate of magnesia Phosphate of lime Phosphate of magnesia Phosphate of iron		2.100
Loss		2.400
Total		1000.000

When a portion of the blood is drawn from the body and allowed to rest, it separates into two parts. The red solid portion which remains at the bottom consists of the fibrin and colouring matter, but the clear fluid which rises to the surface has a specific gravity of from 1020 to 1030, and this, according to Berzelius, consists of the following ingredients :—

Water	905.0
Albumen	80.0
Muriates of potass and soda	6.0
Lactate of soda and animal matter	4.0
Soda, phosphate of soda, and a little animal matter	4.1
Loss	.1
	1000.0

This analysis in the outline is probably correct, but we know that there is also in the serum air, or a gas, an aromatic principle, and an oily substance, neither of which are here noticed. Besides I shall afterwards endeavour to prove that there is not one particle of either soda or any other alkali in the serum in a free state. A free alkali may be obtained from this fluid after the serum has been exposed to a high heat; but the alkali which is thus obtained existed in the blood, not in a free state, but probably in the form of super-carbonated salts.

So long as the blood remains in the living system, and in active circulation, the whole of its ingredients are equally mixed and mutually in contact with each other; but when a part of this fluid is separated from the body, and allowed to rest, it loses its vitality, and in proportion as this takes place, it begins to coagulate, generally in from two to ten minutes.

The fibrin, the albumen, &c., are naturally solid, and, with the exception of the colouring matter, the whole of the solid ingredients of the blood owe their fluidity, not to the presence of a free alkali, but, as I believe, to the circumstance of their being held in solution by a *saline fluid.* While in the circulation, vitality and the incessant motion of the blood tend to prevent the occurrence of those chemical changes that would be fatal to life; but when a part of this fluid is drawn from the system, exposed to the air, and allowed to rest, it loses its living principle; and then those chemical changes are no longer prevented which are, in reality, the cause of coagulation; consequently, it is not the living principle, but the loss of its vitality, which is one cause of the sudden change which the blood undergoes when drawn from the body.

When we add an acid to the drawn blood, this interferes with the agency of the saline matter, and the whole of the ingredients become solid; for, in this case, even the albumen coagulates, and then there is no separation. The fibrin, however, is less soluble in the serum than the albumen; but still, when we add immediately an extra portion of the muriate or the carbonate of soda, or any of the other alkaline salts that are natural to the blood, even the fibrin does not then coagulate, and the whole of the ingredients remain fluid. But, when the blood is left to itself, without any addition, serum of the common strength does not prevent the coagulation of the fibrin, and then the blood separates into two parts.

The serum is the saline fluid which contains the albumen in a state of perfect solution, and so long as the blood is in active circulation, the fibrin also appears to be dissolved in the serous or watery part; but the fibrin is less soluble in this fluid. It is naturally solid, and when no longer agitated, as it is in the circulating current, it readily assumes its natural form. The colouring matter is the only ingredient which is not soluble in a saline fluid. When allowed to rest it mixes with the fibrin, and the union of these two agents forms the crassamentum, or solid part of the dead blood. This, as we all know, sinks to the bottom, and leaves the serum on the surface, perfectly free from any tinge of the red colour.

Coagulation is the result of these changes, and the first part of the process is the apparent solidity of the whole. This, however, is a deception; for the apparently solid mass contains a large proportion of fluid; but in proportion as the fibrin becomes solid it contracts, and in a short period the serum begins to be forced out, first on the surface, and then round the sides; a part of it covers the clot, and the rest of it occupies that space in the cup which is left by the contraction of the solid fibrin.

In the act of coagulation a fine film forms on the surface of the stagnant blood. This film, however, is soon ruptured by the fluid in various places, and the serum appears first in drops, but as these increase in size they unite, and at last, after an interval of many hours, the whole of the saline fluid is forced out, and remains separated from the clot only by the fine film on the surface of the crassamentum.

When we allow the blood to coagulate, and then, before the commencement of the separation, make an incision into the clot, when it is just firm enough to enable us to cut it with a knife, we find that it is, at this period, all equally red; that is, it is all red, so long as the salt serum is still in immediate contact with the whole of the colouring matter. But when we allow the coagulum to remain undisturbed, and examine these two portions of the blood (after the separation is fairly effected), we find that every particle of the saline matter has combined with the serum, and now that the internal or central part of the crassamentum is no longer in contact with this saline fluid, the whole of the colouring matter is perfectly black, except a thin stratum on the surface, which possesses a rich scarlet or arterial appearance.

There are two causes which appear to be concerned in the production of this scarlet colour on the surface of the clot. In the first place, the removal of the cause of the impurity, from the exposed surface, by the oxygen of the air; in the second, the circumstance of its being covered, and in contact with, a saline fluid: for, without the agency of this, neither the removal of the impurity, nor the addition of oxygen, can produce the arterial tint, or even redden the colour on the surface of the crassamentum.

It is, I believe, a common opinion, that the arterial colour of the blood is produced by the absorption of oxygen into this fluid; I shall afterwards, however, endeavour to prove that this is not only an error, but one that has been the cause of great mortality in the practice of medicine. When oxygen is brought into direct, or even into indirect, contact, with venous blood, it instantly reddens the colour. It is not, however, the absorption of oxygen which causes this change; for it is not by addition that this gas produces its effect in brightening the blood, for scarlet is the natural colour of the vital current, and this it owes to another cause. I have ascertained, by numerous experiments, that all the acids blacken the blood, and my conviction is, that carbonic acid, and not carbon, is the cause of the dark colour in the venous circulation. Oxygen however possesses, as I shall afterwards prove, a powerful attraction* for carbonic acid; and when venous blood is exposed to the air, either in the lungs or out of the body, oxygen brightens its colour, *not by addition*, but by *attracting* or removing the carbonic acid from the venous blood, and this becomes bright, exactly in proportion as it loses that which had been the cause of its dark hue. In a high temperature the acid is rapidly removed by the oxygen; when this is effected, the blood is purified, and instantly recovers its natural or scarlet appearance. Oxygen, however, is essential to life; for, without this, the heavy deleterious gas, which is the cause of the impurity in the venous circulation, would not be removed in the pulmonary organs. But the scarlet colour exists in the blood independent of oxygen, or, at all events, oxygen of itself cannot produce either the red or the arterial appearance, for when we cover the crassamentum, when it first coagulates with a layer of distilled water, or any other fluid which does not contain saline matter, the acid may be removed by the oxygen or absorbed by the water, but the colour becomes darker than it had been before. On the other hand, when we immerse the black and saltless crassamentum in any clear *saline* fluid, the colour instantly changes from dark venous to a bright arterial; and when the fluid which we use is sufficiently impregnated with saline matter, this change is produced *when we make the experiment, as I have frequently done, even in an atmosphere of carbonic acid.*

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* This, of course, is not the effect of any chemical attraction; what it is will be considered hereafter.

When the crassamentum remains covered with its own serum, it retains for a time its arterial colour; but when we add an extra portion of saline matter to the natural serum, the surface of the clot instantly becomes much brighter than it had been before, that is, so long as it was merely covered with its own serum, which was less impregnated with saline ingredients; or, when we cover the crassamentum, the moment it coagulates with a clear but strong saline fluid, the colour instantly changes from purple to scarlet.

If we cut out a part of the red clot when it first coagulates, it is then, as I have said, all equally red. When we immerse this red clot in distilled water, it becomes black; or if we place this portion of red coagulum on a table, the serum escapes from the sides and lower part of the clot; while that part of the upper surface of the crassamentum which is left in immediate contact with the air, or even with pure oxygen, becomes black exactly in proportion as the serum falls down from the surface and escapes from the clot. Now, from this simple fact, it is very clear that it is not the oxygen of the air, but the serum, or some of the ingredients in this fluid, which are the cause of the red hue of the colouring matter, and, consequently, of the red colour of the blood.

The serum is composed of water, albumen, saline matter, an aromatic principle, and a small proportion of an oily fluid. Mr. Brande has also found indications of iron in the colourless part of the blood; but this, however, cannot be the cause of the red colour, for neither iron nor any of its oxides possess the power of reddening the blood, or of restoring the red colour to a dark clot, which has become black from the loss of its serum or immersion in distilled water. The peroxide of iron has lately been supposed to be the cause of the red colour; but this cannot be the case, for whatever the colouring matter may be, even when it is quite black, it possesses the property of striking a red colour with any of the neutral salts, which is not the case with iron or any of its oxides. I may also add, that the sulpho-cyanic acid strikes a red colour with the peroxide of iron; but this acid does not redden the black colouring matter; on the contrary, the sulpho-cyanic, like all other acids, blackens even the reddest blood.

It is evident, from what I have stated, that some of the ingredients in the serum must be the cause of the red colour; and as neither water, albumen, oil, nor iron possesses the power of reddening the blood, we may conclude, even *à priori*, that the serum owes this property to its saline ingredients; and that such is the fact can be proved by the most unequivocal experiments.

Mr. Brande considers the colouring matter as a peculiar animal substance; and this opinion, perhaps, comes nearer to the truth than any of the others that have yet been brought forward. It is probably a black, carbonaceous matter, which is formed at the same time with the carbonic acid; but, notwithstanding all that has been written about this agent, we are still in the dark with respect to its real nature, for it possesses properties which are peculiar to itself. Some of these are, however, sufficiently known. We know, for example, that it is not essential to the blood, and that it exists in the greatest abundance in the warm-blooded animals; we know, also, that it dissolves readily in fresh water, but is not soluble in a saline fluid; we know that it is decomposed by chlorine, and that the red colour of the blood is destroyed both by acids, alkalies, the electric fluid, and most of the poisons; we know, also, that black is the natural hue of the colouring matter. But the most remarkable, and perhaps the most important of all the properties of this agent, is the power that it possesses of changing from black to scarlet either when we apply white solid saline matter to its surface, or when we immerse it in a clear solution of any of those salts which are formed with the acids that have either an alkaline or earthy base. Now, as we know of no other black substance that changes from black to scarlet the moment we immerse it in a clear saline fluid, we may infer that the colouring matter is neither an oxide of iron, nor any other agent with which we are yet acquainted. But whatever it may be, it is evident that it owes its red colour in the circulating current to the salts of the blood; and, out of the body, this black agent, when deprived of its salt, is so delicate a test for the presence of saline matter, that its colour changes, in a saline fluid, even when the salt is not perceptible to the taste.

The muriate of soda is the principal saline ingredient in the blood; and, in all civilized countries, this is so essentially necessary in our nourishment, that salt is as regularly put upon the table as the food itself; but, independent of this, every article of nourishment that we use contains either salt or the basis of saline matter, which after a time is converted into neutral salts, that enter the circulating current. It appears, also, from some interesting experiments of Dr. Prout's, that animals possess the power of generating certain salts, independent of the supply which they receive with their food.

It is well known that blood may perform its functions without colouring matter; but, in every part of the world where healthy blood has been analysed, it has invariably been found to contain a given proportion of certain salts. This is not accidental; for they are never absent from the blood of health, and without these, the blood can no more perform its functions, and support life, than air without oxygen can purify the vital current in the pulmonary organs. It is also well known how important the salts are as chemical agents; and, as Nature does nothing in vain, these active agents would not exist so invariably, and that, too, in a given quantity, in the vital fluid, unless they were placed there for some important purpose; and there are many facts which induce me to believe that they are even more essential to the healthy state of the blood, than either the colouring matter, the fibrin, or the albumen: for these may be diminished, but still the blood will perform its functions; but the salts are essential; for, as I shall endeavour to prove, they are not merely the cause of the red colour, but also of the fluidity of the solid ingredients, and one cause of its stimulating quality. These add also to the power which it possesses of preserving itself, even in a temperature of all others the most favourable to the putrefactive process; but when these are lost, or greatly diminished, as in the last stage of the yellow fever, in cholera, &c., the blood becomes black exactly in proportion to the diminution of its saline matter; and when this is diminished to a certain extent, the vital current becomes so vapid as to be totally incapable of stimulating the heart. Often, even before death, it is in a state of dissolution: after this, in hot climates, it goes fast to decay, and almost instantly becomes putrid. When such blood is exposed to the air, *it does not become red;* but when we add a small portion of saline matter, even to this black and dead fluid, the scarlet or arterial colour is immediately restored, and after this it is not only florid, but less offensive than the same blood to which we have not added any saline ingredient.

When we add a solution of any one of the neutral salts, even to the darkest venous blood, this increase of saline matter almost instantly reddens the colour; and as soon as the carbonic acid is removed by the oxygen of the air, the colour of the whole becomes much more highly arterial than it is in blood from the same vein to which no addition has been made.

When we leave the crassamentum of healthy blood completely covered with its own salt serum, it retains its arterial appearance on the surface for several days; but it soon becomes black when we take the crassamentum out of the serum, so as to expose its surface directly to the air. From this it is clear that oxygen is not the cause of the red colour, for without serum it becomes black, even while in direct contact with the purest air. It is true that, in the process of respiration, after the acid is removed, there is a portion of air taken into the circulation: this, however, is to serve another purpose, and not to brighten the blood, for the colour is changed before the air is attracted into the current.

When we cut out a piece of the red crassamentum from healthy blood which has just coagulated, and immerse this in distilled water, the water rapidly attracts the saline ingredients out of the clot. In proportion as this takes place the colour changes, and in a short period it becomes perfectly black: from this we may infer, that black is the natural colour of the colouring matter; for it is only red so long as it remains in immediate contact with a saline fluid. When we take this black clot out of the distilled water, and expose it directly to the air, it remains black; or, if we immerse it in a jar of pure oxygen, the oxygen can now no more redden its colour, than it can change the colour of the blackest ink. There is but one way by which the red colour can be restored, and that is neither by air, iron, nor oxygen, but by restoring to the blood the saline matter which it has lost; and when we sprinkle or rub a small quantity of the muriate or the carbonate of soda, or any of the neutral salts, on the black clot, not merely the red, but a colour that is highly arterial, is immediately produced; or when we make an artificial serum, by impregnating water with any of the neutral salts, and then take the black clot out of the clear fresh water and immerse it in this equally clear saline fluid, it is immediately changed from black to a bright red colour. When we take this scarlet clot out of the saline fluid, and immerse it again in distilled water, it soon becomes black; but when we remove it from this, and immerse it again in the clear saline fluid, it again changes to a rich scarlet; and this we can repeat, even with the same clot, as often as we please. Now, as these experiments prove that, when the saline matter is withdrawn from the blood, it becomes black, and when this is restored it recovers its arterial colour, we may then, I believe, safely infer that the saline matter of the vital current is the true cause of the red colour of the colouring matter, and, of course, of the red colour of the blood.

A clear fluid that is more impregnated with salt than common serum will brighten the colour, even independent of the removal of the acid. This I ascertained by many experiments, in an atmosphere of natural carbonic acid, so strong, that it was fatal to rabbits, &c. in less than three minutes. It is also true, that acids, alkalies, electricity, poisons, but particularly those aëriform poisons which are, in reality, the remote cause of the malignant fevers, and, in short, every thing that either decomposes or interferes materially with the agency of the saline matter, destroys the red and gives a black colour even to arterial blood.

Oxygen is evidently not the cause of the red colour; for it cannot redden a black clot which has entirely lost its saline impregnation. Oxygen, as I have said, can only indirectly redden the venous blood by the sudden removal of the carbonic acid; for it is the salts, and not the absorption of oxygen, which is the true cause of the scarlet colour in the arterial system. Neither do the salts produce this colour by communicating oxygen to the blood, for if they do, why is it that the blood is instantly reddened by the muriate of soda, the sulphate, the nitrate of potass, and other salts of the same class which do not contain one particle of oxygen in a separable form? and why is it that these salts redden the current in the living system, and after a time pass through the circulation unchanged? for they are not decomposed either in the gastric organs or by the vital principle; a part is retained in the circulation, and the excess is found in the secretions in their original undecomposed state: or, if oxygen does redden the blood by addition, why is it that acids and other agents, which contain oxygen in a separable state, darken the colour, and convert even arterial blood into a fluid that is, literally speaking, as black as ink?

As I have said that the acids blacken the blood, it may be proper to add, that there is an exception to this rule; for as the carbonates are weaker salts than those that are formed with fixed acids, and as the alkaline carbonates form a considerable portion of the natural saline matter of the blood, it follows that, when we add an acid solution that is just strong enough to convert this weaker into a stronger salt, we then brighten the colour. It is for this reason that a salt with a slight excess of acid increases the arterial appearance. It is also for the same reason that there is generally an increased glow of red when we first mix any of the acids with the warm blood; but if we add a very small proportion more of a fixed acid than is just sufficient to convert the alkaline portion of the carbonates into neutral salts, the red colour is so irretrievably destroyed by the excess of acid, that with the addition of a little water, the whole is converted into a fluid which exactly resembles the black vomit.

It has been asked, if the saline impregnation be the cause of its red colour, why is it that the blood has a rich scarlet appearance in the arterial circulation, and a dark or Modena red in the venous, which contains as much saline matter as the arterial blood? Now, in the first place, it is not quite true that the venous contains as much saline matter as the arterial; for, with one exception (the bile), the whole of the secretions derive their salts from the arterial circulation, and the venous blood, for example, which is returned from the kidneys or the gastric organs, must naturally contain less saline matter than the arteries which supply these organs with blood, from which they derive the whole of the saline matter contained in the secretions. We know, also, that there is a quantity of saline ingredients carried to the muscles and to most of the other solids, and that this is returned, not by the veins, but by the lymphatic vessels. All this, however, is not enough to account for the great difference in the colour of the two fluids: we must, therefore, look for some other cause.

Facts, ascertained by experiments, are, or ought to be, the chief foundation of theory; but, unfortunately, from the present imperfect state of animal chemistry, when minute experiments are made even by the ablest chemists, the results which are obtained are often essentially different, and, in some cases, diametrically opposite. It is when we find ourselves entangled in such difficulties as these, that we are forced to rest our belief less on the result of actual experiments, and more on those facts which are most in accordance with our own observations; but, above all, on the common sense of the argument at issue.

We are told, for example, by one class of chemists, and I believe with truth, that a large proportion of carbonic acid can be obtained from venous blood, while others assert that this acid does not exist in the venous circulation; but that carbon is the cause of the dark colour in the venous current, and that the acid is formed in the lungs, either in the air-cells or in the blood itself.

One of the chief arguments which has been brought forward against the existence of carbonic acid in the venous circulation, is the difficulty of procuring this gas from the blood when it is placed in a vacuum. The blood certainly possesses the power of attracting carbonic acid, that is, when there is no external oxygen present as in the extreme circulation; but the attraction which oxygen possesses for this gas is much greater than that of the blood, and when oxygen is brought into contact either directly or indirectly with blood or any other fluid that contains carbonic gas, the acid is immediately removed by the oxygen and diffused in the air.

When venous blood is exposed, even for a short period, to the atmosphere, the rapidity with which the oxygen attracts the acid from the warm fluid is such, that a great part of the gas may be removed before it can be put into a vacuum; but when proper pains are taken not to expose the blood to the air, a large proportion of carbonic acid gas may then be obtained; and that such is the fact has been fairly proved by the well-conducted experiments of Mr. Brande, Vogel, Bauer, &c. &c.

Though the celebrated experiments of Dr. Edwards do not prove that the carbonic acid is merely exhaled in the lungs, yet there is one point of view in which I consider these experiments as of great value; for even if they stood alone, they are of themselves sufficient to prove the existence of carbonic acid in the venous circulation: for from these experiments it is very clear that this acid is not formed in the lungs, by the union of the carbon of the venous blood with the oxygen of the air. Hydrogen may *attract* carbonic acid, for a time, from the venous blood in the pulmonary organs, yet hydrogen can have no share in the formation of that gas, either in the lungs or anywhere else: consequently from the experiments, it is evident that the immense quantity of acid which was attracted from the lungs of these animals, had been formed, not in the pulmonary organs, but in the extreme circulation, in every part of the living body, by the carbon of the solids attracting to itself the oxygen which had existed in the blood, before the animals had been placed in an atmosphere of hydrogen gas.

But the great difficulty that exists even to this day against the belief in the existence of carbonic acid in the venous circulation, is the argument which has been brought against it by Dr. John Davy, and several others; that is, as they say, the existence of a free alkali in the blood; consequently, as they state it, a free alkali and a free acid cannot exist together in the same fluid. This argument, however, is much more apparent than real; for notwithstanding its plausibility, it may be proved, that its very basis is founded upon error.

Dr. Davy informs us that his objection must be universally admitted. 'It cannot,' he says, 'be controverted; all those who have examined the blood are agreed that it contains free alkali, and that in a notable proportion. This being granted, is it not contrary to all the rules of chemical science to infer that blood should contain a free acid, &c. &c.?' Now, notwithstanding all this, I am not quite sure that the existence of this free alkali in the blood has been fairly proved. It is true that a free alkali may be obtained from the serum after it has been exposed for some time to a high heat; but this is no proof that it existed in that form previous to the analysis. It is also true that the serum, particularly after it has been exposed for some time to the air, produces an alkaline re-action on the test paper; but I believe that this effect is produced, not by a free alkali, but by the alkaline carbonates which form a considerable part of the natural saline matter of the blood. The carbonates give a brown colour to the turmeric paper, as well as the free alkalies; and, although carbonic acid may not be able to remain in a free state in the same fluid with a free alkali, yet this gas may very well exist in a free state in the same fluid with the alkaline carbonates. But though the alkalies and the carbonates have a similar effect on the turmeric paper, yet in their other properties they are essentially different from each other, and above all with respect to their effects on the vital fluid; for the carbonates give a beautiful arterial colour to venous, while the alkalies make even arterial blood much blacker than it had been before the addition.

The saline water of Saratoga, in the United States of America, contains, like the blood, a large proportion of muriate of soda, as well as of the alkaline carbonates, but not one particle of a free alkali.

At the moment that this water is first taken from the spring it is supersaturated with free carbonic acid; but, notwithstanding this excess of both acid and carbonates, when the water is just taken from the fountain, it does not redden the litmus, neither does it give a brown colour to the turmeric paper· yet when this water has been exposed for a short time to the air, the acid is soon removed, and when this is effected, the water then gives a decided brown colour to the alkaline test; but not until the greater part of the free acid has been removed by exposure for a short time to the atmospheric air, or when the acid is expelled more suddenly by a gentle heat.

If there be no carbonic acid in the blood, and if the serum owed its property of changing the colour of the test to a free alkali, it ought then to possess this power in as great a degree when it first separates as it does after it has been exposed for some time to the atmosphere. But this is not the case; for healthy serum, when it first separates, seldom changes the colour of turmeric paper. It is only after exposure to the air, and after the removal of the carbonic acid, that the carbonates in the serum then give a decided brown colour to the turmeric paper.

Litmus paper reddened with an acid is a more delicate test, either for the free alkalies or the alkaline carbonates, than turmeric, and on this, when it first separates, the serum generally produces a slight alkaline re-action; but this quality is at first so faint, that I have scarcely met with an instance where the alkaline quality of healthy serum at the moment when it first separates from the crassamentum was so strong as to change the colour of the turmeric paper; and when we exclude the serum from the air it does not acquire this power, at least for a long period. On purpose to ascertain this, a phial was filled with recent serum and excluded from the air for nearly a week, and when the stopper was first withdrawn, the serum did not produce any change of colour in the turmeric paper.

If there be any free carbonic acid in the serum when it first separates, this will naturally soon be removed by exposure to the air; and any portion of acid that may remain after this, is that which is held in firm combination with the alkalies. To ascertain whether there be or be not a portion of combined carbonic acid in the blood, I added a few drops of concentrated sulphuric acid to a phial of serum; the fixed acid immediately coagulated the albumen, and during the coagulation there was a decided effervescence arising from the escape of the carbonic acid. Now as acids do not effervesce with free alkalies, the effervescence in this case can only be produced by the sudden decomposition of the alkaline carbonates.

All the acids blacken the blood, and many facts induce me to believe, that carbonic acid is decidedly the true cause of the dark colour in the venous circulation, and when this is removed by the air, it is neither oxygen nor iron, but the saline ingredients of the blood which then give it its scarlet appearance.

I have stated also that all the free alkalies destroy the red colour of the colouring matter; but when we add an excess of any of the alkaline carbonates, and mix them with the blood, its colour is instantly changed from a dark venous to a rich arterial. Now, if the blood contained an excess of free alkali, its colour would not be brightened by the removal of the carbonic acid; but as we know that, when the acid is removed, its colour is instantly changed from dark to scarlet, we may infer from this fact alone, that the blood contains a large proportion of the alkaline carbonates and other salts, but not one particle of an alkali in a free state; for this, on the removal of the acid, would blacken the colour in place of changing it from venous to arterial.

The belief that a free alkali exists in the blood is opposed by the facts which I have stated, but the existence of carbonic acid in the venous circulation has been clearly proved by numerous and well-conducted experiments. We may, therefore, change the form of the argument, and say, that the alkalies can no more exist in a free state in the same fluid with a free acid, than carbonic acid can exist in a free form in the same fluid with a free alkali. Now, as it has been proved, that free carbonic acid does exist in the venous circulation, we may safely conclude that the agent in the serum, which gives a brown colour to the turmeric paper, is not a free alkali, but one, or perhaps more, of the alkaline carbonates.

From the facts which I have stated, we may now infer that scarlet is the natural colour of the vital current, and this it derives from the action of the salts on the hematosine, or colouring matter of the blood. Carbonic acid, however, is the cause of the dark colour in the venous current, and when this is attracted, or removed by the oxygen of the air, the colour almost instantly changes from a dark purple to a bright scarlet, such as we observe it in the arterial circulation.

ANIMAL HEAT.

The doctrine of animal heat is still far from being settled; but it is evident, that wherever the carbonic acid is formed, there the heat must also be evolved. The generally received opinion, I believe, is, that this acid is formed in the lungs; but, from many facts, I have long ago been led to the belief, that the acid is formed and the heat is evolved, not merely in the lungs, but all over the animal frame in the extreme texture, where the blood is converted from arterial to venous. I was first led to form this opinion in consequence of certain phenomena that occur in the cold stage of the marsh fever, from which it is very evident that the heat is evolved, not in the lungs, but in the extreme vessels, all over the living body.

The force of the circulation, and the quantity of heat evolved in the system, invariably keep pace with each other. The extreme vessels, which are probably most concerned in the evolution of heat, are more under the influence of the brain than the heart itself; and when this organ, as in fever, is supplied with impure or diseased blood, its functions are deranged, and the nervous system suffers in common with every other function of the animal body. In the commencement of the disease, the extreme vessels appear to be the first part of the vascular solids which feel the effects of the remote cause; their action becomes languid, and in proportion as this takes place, the animal heat ceases to be evolved in its usual quantities, the temperature of the blood in the extreme vessels soon falls, the skin and the extremities become cold, and when the blood, which has not been properly heated in the extreme circulation, arrives at the vital organs, the whole system is then chilled into a state of complete torpor.

Even in mild fevers, such as the marsh intermittent, the patients sometimes die during the cold stage; but, in general, re-action soon comes on, and when it does, it commences first in the extreme vessels; and there is a period, in such cases, immediately after the commencement of the increased excitement, when the skin feels burning hot to the physician, while the patient himself still feels deadly cold, and the internal vital organs only recover their temperature when the blood that has been heated in the extreme circulation arrives at the heart, from which it is sent first to the lungs, afterwards to the left ventricle, from this to the brain, and all over the body. It is then that the re-action and the heat become universal. Now, as the blood in the extreme vessels all over the system is the first to lose its temperature, in the cold stage of fever, and the first to become warm in the hot stage of the disease, we must look, not to the lungs, but to the chemical changes which the blood undergoes in the extreme texture, as the cause both of the generation and the immediate evolution of that heat which enables all warm-blooded animals to maintain their natural high temperature, even when surrounded by the coldest air.

We are told by one writer, that the heat which maintains the temperature of the body, is produced by an action; and that this heat is evolved in the stomach. We are informed by another, that it is a secretion, and produced by the nerves; while others maintain that it is the effect of the vital principle; and that too, even after it has been proved that the temperature can be kept up and increased in animals, after their heads have been separated from the body.

The brain and the nerves are necessary for other purposes, but they are not essential for the evolution of heat. Caloric is evolved in common combustion, independent of nerves; and by a similar process, heat is often evolved in the living animal without any assistance from the nervous system. A limb, for example, that has been completely paralytic for many years, can generate heat, long after all communication with the brain has been completely destroyed. And as this is the fact, we must look, not merely to the brain, or the nervous influence, when we try to explain the cause, why the more perfect animals can maintain a high temperature, even when surrounded with air that is a hundred degrees colder than the blood.

It is certainly the easiest way of accounting for difficulties by referring them to the nervous system, or the living principle; but this sort of explanation generally leaves us just where we were. The nerves, of themselves, are not the cause of heat, or the cause of impressions; they are merely the medium by which these are conveyed. The nerves may convey the impression of heat to the brain; but these agents cannot of themselves evolve heat in the extreme texture of the living body. When, for example, we tie the large artery leading to a limb, do the nerves, of themselves, though they be left uninjured, keep up its temperature? And were it not for the blood that is carried into the limb by the anastomosing branches, would it not, nerves and all, immediately become cold as death, and speedily die?

The nerves have some share in the mere quantity of heat that is evolved. But even this they possess only indirectly, or inasmuch as they have some influence over the motion of the vessels in the extreme circulation; and, for this reason, in a limb that is completely paralyzed, the heat is not quite equal to what it is in another limb that is not paralytic. But still, in a limb where the nervous power is completely lost, the heat continues to be evolved through life; and, in some cases, as in the hot stage of fever, the animal heat is greatly increased. And if a limb, under such circumstances, can generate even one particle of heat, we may infer, that though the nerves may assist, yet they are not essential in the process which is incessantly going on for the evolution of that heat which is the cause of the high temperature in all those animals which have a double circulation and warm blood.

It was found in Mr. Brodie's experiments that the temperature was diminished even when the respiration was kept up by artificial means. One cause of this is, the circumstance that the extreme vessels where the heat is formed are more under the influence of the brain, than either the heart or the great arteries; and when the head of an animal is cut off we can scarcely expect that the blood will circulate as freely in the extreme vessels, as it does in the natural circulation of a living animal which possesses its full vitality. But, besides this, it is very evident that, in these experiments, too large a proportion of cold air was forced into the lungs. The temperature of the air that was used ranged from 57° to 64°; that is, from 38° to 45° lower than the natural heat of the blood of the animals. Rabbits breathe from thirty to thirty-five times in a minute, but the lungs were inflated much more frequently than this, and in one of the experiments they were inflated at the rate of from fifty to sixty. We know also that the lungs can contain a much greater proportion of air than is ever used in the process of natural respiration; and when the air-cells are more frequently and more fully distended with cold air in a dead animal, than ever they are in the common respiration of one that is alive, under such circumstances we can scarcely be surprised that the blood in the lungs should become cold, and that this cold blood should diminish the heat in the whole body; particularly in a dead animal, where vitality is not present as an agent in the process, and where the usual quantity of heat cannot be evolved in the extreme circulation.

These experiments, however, have since been repeated by Dr. W. Philip, L. Gallois, &c.; and when pains were taken by these gentlemen not to force too much cold air into the chest, it was found, that even dead animals possessed the power of evolving heat. Now, if we can keep up the temperature in a dead animal, even after the head has been separated from the body and, if a paralytic limb can not only evolve, but even increase its heat, as in the hot stage of fever, we may then safely infer that the generation of animal heat is neither a nervous action, nor a mere vital process, but produced by a chemical change which is incessantly going on, not in the lungs, but in the extreme textures all over the living body.

It has been ascertained by the experiments of Dr.

Prout, and confirmed by those of Edwards, &c., that the quantity of carbonic acid thrown off by the lungs varies considerably, from various causes. It is greatest about mid-day, least at midnight; greater in the prime of life than in youth or in old age. In fact, whatever increases the force of the circulation in the extreme vessels, increases, at the same time, the quantity of heat that is generated in the system, and, of course, the quantity of carbonic acid which is evolved in the lungs.

Every part of the system evidently provides its own heat; and that the quantity generated depends upon the rapidity with which the blood circulates through the extreme vessels, is very evident from what occurs during the process of inflammation. In this disease, though the respiration is not more frequent than in health, yet when the circulation is greatly increased in any one part of the body, the quantity of heat evolved in the affected part is infinitely greater than it is in the rest of the system, where the blood is circulating at its usual rate. It is also a fact, that during the hot stage of the climate or endemic yellow fever of the West Indies, even when the general excitement is very great, the respiration is scarcely increased; but the quantity of heat evolved all over the body is, at one period of the disease, so great, that it is almost painful to touch the skin. It is also true, that in the last stage of this fever, when the saline ingredients of the blood are nearly exhausted, the patients become weak, the circulation flags, and the respiration increases exactly in proportion as they become cold and exhausted. There is then little heat evolved in the extreme textures, there is little carbonic acid added to the venous circulation, and the blood becomes black, not from an excess of this agent, but from the decomposition or almost entire loss of its natural saline ingredients.

It is well known that the temperature of a surface in a state of active inflammation is often several degrees higher than that of the arterial blood with which it is supplied. From this fact alone, it is very clear that the extra heat cannot be produced merely by the addition of a colder fluid to the inflamed part; and if the blood can be heated, even at a great distance from the lungs, we may infer that every part of the living body possesses an apparatus within itself, which enables it to maintain its high temperature by the incessant generation of its own caloric.

If the animal heat were generated in the lungs, we might then, at will, increase the temperature, merely by breathing for a time as quickly as possible; a person, however, who is surrounded by a cold medium cannot keep himself warm merely by rapid respiration: but by exercise or motion we increase the action of the extreme vessels; by increasing this, we increase the temperature of the whole body; and in this way the blood is enabled to maintain its own high heat, even when the surrounding medium is intensely cold.

It is almost incredible how small a quantity of nourishment is necessary to enable the cold-blooded creatures to support life. They breathe slowly *, their blood is seldom renewed, and the small quantity of food which is taken into the system is expended almost entirely for the nourishment of the solids; and for this purpose but very little appears to be required, as their solids, like the blood, are but seldom changed. In some of these creatures one good meal in three months is quite sufficient, while others can live on less; yet, even with this, they are active, muscular, and strong, in proportion to their size. I have seen, for example, a large rattlesnake, which, it was said, had not tasted either food or water for nine months, yet he was plump, active, and venomous as those in the woods.

On the other hand, the quantity of food which is consumed by all the warm-blooded animals, is evidently much greater than is necessary for the mere nourishment of their solid structure; those animals, too, which have the highest temperature, invariably use the largest proportion of food, and when this is withheld they soon die. Ducks, fowls, &c., are (when they can get food) almost constantly eating; and as all animals that have warm blood use much more food than is actually necessary for the nourishment of the solids, or the supply of the secretions,

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* I once counted the respiration of a young alligator, and though he was agitated from having been laid hold of, he breathed only from three to four times in the course of a minute.

we may infer that a great proportion of this is used as so much materiel for the incessant evolution of that animal heat, which is as necessary for their existence as the air which they breathe.

While in health, a human being generally respires about twenty times in the course of a minute; but those animals that have a higher temperature breathe much faster than this. In these, the grosser ingredients of the blood are frequently renewed, and the solids themselves are constantly undergoing an incessant and rapid change, which does not appear to be required in cold-blooded creatures, or to be otherwise necessary in those that have warm blood, except for the purpose of evolving heat.

The materiel of carbonic acid exists in the blood; the carbon is derived from the nutritive fluid, which enters the circulation through the medium of the thoracic duct; the oxygen is attracted into the circulation in the lungs; while the vital principle of the blood is, probably, derived from another and a higher source; but its carbon, and the other grosser ingredients, are derived from the chyle. It is by this combination of chyle, air, and vitality, that the blood is formed; the arterial blood, however, from the time that it leaves the lungs, suffers no change in its properties until it arrives in the extreme textures all over the system; for as I have said, even in the most minute arteries, the blood is as highly arterial as it had been in the left side of the heart, while the blood, even in the smallest veins, is also completely venous: consequently, the change from arterial to venous, evidently takes place only in the extreme texture; this change is evidently produced by the addition of carbonic acid, and we all know that whenever or wherever this acid is formed, heat is invariably generated; now as the carbonic acid is evidently formed in the extreme texture, it is very clear that there also the animal heat must be evolved.

When blood is exposed to destructive analysis, carbon is found to be the largest constituent; it is, also, the chief ingredient in the solid structures. We know also that the whole of the solids are formed from their nutritive fluid; but in all those animals that have a double circulation and a high temperature, even the solids, like the blood itself, are incessantly undergoing a variety of changes, particularly in the extreme texture all over the body, where the arterial circulation ends and the venous begins. It is there, by a process as if for the express purpose of evolving heat, that in all warm-blooded animals, a portion of the carbon of the old solids is liberated or evolved in a free state; it is there that this fixed agent attracts to itself a part of the oxygen from the arterial blood. It is there that the animal heat is evolved, in consequence of the chemical combination of these two agents; it is there that the carbonic acid is formed and added to the venous blood, as a necessary evil; for this is the result of that process which is so essentially necessary, in the more perfect animals, for the incessant generation of that heat, without which they can no more exist, than they can live without food, or breathe without air. The carbonic, however, like all the other acids, blackens the blood; and when this is added to the venous circulation, it not only darkens its colour, but changes its properties, and renders the fluid which contains it so impure, that it is totally incapable of supporting life. It is for this reason that all the more perfect animals have a double circulation; and the whole of that blood which is rendered impure by the evolution of heat, is immediately sent to the pulmonary organs, where the blood is purified by the instantaneous removal of the carbonic acid by the oxygen of the air, and becomes arterial, in consequence of having lost that acid which had been the cause of its impurity and dark colour in the venous circulation.

The opinion that the carbonic acid is formed, not in the lungs, but in the circulating current, was first, I believe, brought forward by La Grange, and afterwards supported by Hassenfratz. These writers believed, however, that the addition of oxygen was the cause of the arterial colour, and that the carbon of the blood itself combined slowly with the oxygen, and that this process commenced, even in the large arteries, as soon as the blood leaves the left side of the heart. To prove this, Hassenfratz enclosed arterial blood in glass tubes, which were then hermetically sealed. It was found that, after a considerable time, the arterial blood became dark; and from this he inferred that the carbon, not of the solids in the extreme texture, but of the blood itself, was the cause of the change from arterial to venous.

It is true that dead arterial blood will, in time, become like venous, whether it be or be not exposed to the air; but this depends upon a chemical change, which takes place very slowly, and is very unlike that sudden process which occurs in the living body, when the carbon of the old solids attracts the oxygen from the arterial blood to itself, and in this way instantly evolves heat, and forms the acid in the extreme texture. Besides, it is very clear that this change does not take place slowly and equally in the whole round of the circulating current; for, as I have stated, the blood is highly arterial in the most minute artery, while even in the smallest vein it is completely venous; consequently, this change only takes place where the arterial circulation ends and the venous commences.

We know that there is not one particle of free carbonic acid in healthy arterial blood; we know also that it exists in the venous, even in that of the smallest veins. From this it is very evident that the venous blood must have acquired this addition not in the larger vessels but in the extreme texture; and as the acid is formed there, we may infer with propriety that there also the animal heat must be evolved.

The chemical union of oxygen with carbon is, probably, the chief cause of animal heat; but the evolution of this is not exactly equal in every part of the living body. An extra quantity of caloric, for example, is required in the lungs, in consequence of the peculiar functions of these organs; for, from the large quantity of air which is taken into the chest in cold weather, it is necessary that the temperature of this air should be suddenly raised to 100°, on purpose to enable the oxygen rapidly to remove the acid from the circulation; there appears, however, to be a peculiar provision for the sudden evolution of this extra proportion of heat, which is necessary from the nature of the functions of the pulmonary organs.

With respect to capacity for caloric, I believe that the fact is exactly the reverse of that which has been stated by Dr. Crawford, and that the venous blood has, in reality, a greater capacity for heat than arterial. This has been partly proved by the experiments of Dr. J. Davy; and further proofs may still be produced.

When Dr. Wilson Philip passed the galvanic fluid through arterial blood, heat was instantly evolved, and the colour was immediately changed from scarlet to black. According to this physiologist, heat is a secretion; and the above experiment has been considered as a proof of the power of the nerves in evolving heat. In this experiment, however, the dark colour of the blood was evidently produced by the sudden decomposition of the saline matter, and the sudden evolution of heat was probably produced by the same cause. This increase of heat, it is said, could only be produced in arterial blood; and if so, this is one proof that the arterial has a less capacity for caloric, and parts with its heat more readily than the venous. Animal heat, however, is not a secretion; for there can now be no question of the fact, that the greater part of the heat which is evolved in the living body, is produced by the union of oxygen with carbon, just as certainly as that this union is the cause of heat in common combustion. The galvanic fluid may decompose carbonic acid, but it can have no share in generating heat, by producing a chemical union between the two agents by which it is formed. The nervous influence may, as we observe in blushing, indirectly increase the quantity of caloric, by increasing the force of the circulation in the extreme vessels; or, when caloric is evolved, the nerves may convey the impression of heat to the brain; but the nerves, of themselves, can no more produce heat than they can cause sound, or give light to the eye in the midst of darkness.

When an alkaline oxide is converted into an alkaline carbonate, its capacity for caloric is also increased. It is also true, that when we apply heat to a carbonate, so as to expel the acid, the alkali recovers its original form, and its capacity for caloric is again diminished. When we dissolve, for example, the carbonate of soda in water, heat is absorbed by the salt, and the fluid becomes cold; but when we dissolve an alkaline oxide, heat is immediately evolved, and the temperature of the water rises, during the solution, several degrees higher than it had been before.

When carbonic acid is added to the circulation in the extreme texture, the capacity of the blood for caloric is also increased, and a part of the heat, which is evolved by the union of the oxygen with the carbon, is absorbed by the venous blood, and retained in a latent form until the acid is removed in the lungs. The latent heat is then evolved, and this is sufficient, not only to heat the cold air in the chest, but also to raise the temperature of the arterial blood from one to two degrees higher than it had been before. This increase of temperature increases the stimulating power, as well as the fluidity of the arterial blood, and renders it more able to perform its important functions in the living system.

The quantity of heat which is evolved in this way depends on the quickness and extent to which the blood is converted from venous to arterial, that is, on the rapidity with which the carbonic acid is removed, and the oxygen is afterwards absorbed into the circulating current. When we expose venous blood in a carbonic atmosphere, the acid of the blood is not removed; there is, of course, no oxygen absorbed, and the blood, under such circumstances, cools nearly as fast as any other fluid of the same consistence; but when we expose it to the air, these changes occur, and it cools more slowly than a common fluid; or when we expose the blood in an atmosphere of pure oxygen, it is instantly and completely changed from venous to arterial, and heat is evolved in proportion to the suddenness and extent of the change. Sir Charles Scudamore exposed two portions of the same blood, under similar circumstances, one to common air and the other to pure oxygen, and, at the end of eight minutes, the temperature of the latter was eight degrees higher than that of the blood which had been merely exposed to the common air.

It is a fact that when the blood contains an excess of saline matter, it adds to its stimulating power, increases the force of the circulation, and a greater proportion of heat is evolved than at other periods. We all know also that when an excess of non-purgative saline matter is taken into the stomach, it is soon followed by thirst, heat, and great excitement all over the body, which continues until the excess be removed by the secreting organs: or, when we inject a saline fluid into the veins of an inferior animal, the action of the heart is immediately increased, and an extra proportion of heat is evolved; but of this hereafter. It is also true, that during severe winters, in the northern parts of America, the farmers can only preserve their sheep, and other domestic animals, during the extreme cold, by giving them large quantities of common salt; and, at this period, the natural craving for salt is so great, that even the wildest become perfectly tame to any individual who has once supplied them with this article, and attempt to follow him wherever he goes. When supplied with this, they are enabled to resist the most intense cold, but when it is not given many of them die. The above circumstance was mentioned to me by some intelligent agriculturists whom I met with in Canada. It is also true, that some wild animals, at certain periods, frequently risk their lives on purpose to drink the water of saline springs, and can scarcely be deterred from this, even by the presence of human beings.

But, independently of animal heat, it is well known that all living animals possess, more or less, an inherent principle which endows them, to a certain degree, with the power of resisting cold. That this power is not merely dependent on the property of evolving caloric, is evident from the fact, that cold-blooded creatures possess this property in a much greater degree than those animals that have warm blood. We may, therefore, infer, that this power is the effect of the vital principle, for it exists in vegetables as well as in the whole of the animal creation; but it is most perfect in the cold-blooded creatures, who are more tenacious of life, and endowed with animal vitality to a much greater degree than the more perfect animals that have warm blood; and, for this reason, they can, without either air or exercise, continue their existence in extreme cold, and live, under circumstances that would be instantly fatal to those that are more perfect, but much less tenacious of life.

ON RESPIRATION.

It has been asserted, that the purification of the blood is the effect of the vital principle; but if so, then the air must possess life; for Priestley ascertained that a similar change is produced when *dead* blood is exposed to the air, even through the medium of a dead membrane; and how little either the brain or the nerves are essential to this process has been well proved by the more recent experiments of Mr. Brodie, who found that, when respiration was maintained by artificial means, the blood continued to be purified in the lungs, even for hours after the head of the animal had been cut off; and as this fact is unquestionable, we must look, not to the nerves or the living principle for an explanation of this process, but to the agency of some other power.

I have long been convinced that the theory of respiration could not be explained on any principle which is merley chemical; but my attention was first particularly attracted to this subject, while making some experiments on the blood, in the West Indies, in 1827. I then ascertained, by a very simple experiment, that oxygen does possess, as I believe, a powerful attraction for carbonic acid; and this fact appeared to me to remove most of the difficulties which had formerly existed on this subject.

Carbon, as we all know, can only combine with a given proportion of oxygen. Carbonic acid is carbon in combination with the largest proportion of oxygen which it is capable of combining with; consequently, were we to be guided merely by theory, we might infer that oxygen and carbonic acid could have no further agency upon each other. Such, however, is not the fact: for though carbonic acid is much heavier than common air, yet, when we fill a tumbler, or any other vessel, with this heavy gas, and expose it to the atmosphere, the greater part of the acid ascends; and, in proportion as this takes place, the space in the tumbler is immediately replaced by atmospheric air. But when we fill a tumbler with the acid gas, and tie a membrane firmly over the mouth of the vessel, still the acid is removed from the tumbler more rapidly than the air can enter, and the membrane becomes concave from the atmospheric pressure occasioned by the sudden removal of the acid from the glass. Such is the fact, but the cause, whatever it may be, which produces this rising up of the heavy acid, even through the medium of a dense membrane, is, at present, but ill understood. It is evidently, however, not the effect of a chemical process, for there cannot be any chemical affinity betwixt the air of the atmosphere and carbonic acid; neither is the removal of the heavy gas in this instance produced by electricity, caloric, or any other agent with which as yet we are generally acquainted.

The cause, whatever it may be, which removes the acid from the glass is, evidently, some active agent in the atmosphere, and, without the agency of this, vegetables could not live, and the animal creation could not exist. But the very existence of this active cause has scarcely been suspected, and its effects, important as they are, have been, until lately, almost overlooked; but of this hereafter.

When we add a clear saline fluid, even to the darkest blood, the colour is immediately changed from venous to arterial; but when we saturate the same saline water with carbonic acid, the first effect of this acidified saline solution is to make the venous blood considerably darker than it had been before. This dark colour, however, continues only for a short period, for when exposed to the air, the colour changes in proportion as the acid is removed by the oxygen; and, in a high temperature, the acid is so rapidly attracted, that even in a few minutes the blood that was so black is changed, by the additional proportion of saline matter, into a fluid that is of a much brighter scarlet than common blood ever assumes when merely exposed to the air.

The natural saline waters of the Saratoga springs contain, like the serum of the blood, a large proportion of saline matter; and in both these fluids the muriate of soda is the chief ingredient. The water contains also other salts of soda, lime, magnesia, and a minute proportion of iron; all of which exist in the water in the form of carbonates. One gallon of water from the Congress spring, is said to contain six hundred and seventy-six grains of these salts, of which four hundred and seventy-one are muriate of soda, and those remaining are carbonates. While in the fountain, this saline water is saturated with carbonic acid; and, when just removed from the spring, one gallon is said to contain three hundred and forty-three cubic inches of this gas in a free state. I added a portion of this acidified saline water to some warm venous blood, which had just been drawn from a healthy person: its first effect was to darken the colour; but, in a few minutes, the appearance of the blood was completely changed; for, in proportion as the acid was removed by the air, the colour suddenly changed from almost black to a tint that was highly arterial.

In another experiment, I exposed the recent water to a gentle heat, so as to expel the whole of the free acid; and when a small portion of this was added to a quantity of warm venous blood, the colour was almost instantly changed from dark venous to a rich scarlet.

From these facts, it is very clear that the presence of free carbonic acid in water prevents even the saline matter from communicating the arterial colour to the blood, so long as the fluid contains an excess of free acid. When this acid is added, even to arterial blood, the colour instantly changes from arterial to a dark venous or Modena red; but when this gas is removed by the air, the saline ingredients act then with force, and the colour suddenly changes from venous to arterial.

The *fixed* acids irretrievably destroy the red colour of the colouring matter; but the acid properties of carbonic acid are so weak, that an atmosphere of this gas, even so strong as to kill an animal in two or three minutes, scarcely gives a red tinge to the litmus paper. When this weak and insoluble acid is added to arterial blood, it darkens the colour and renders it venous; but the carbonic does not, like the fixed acids, destroy the structure of the colouring matter. A part of it exists combined with the alkalies; and the carbonates, like the other salts, redden the blood, but the free acid darkens the colour. It is not soluble, however, in the circulating current; but exists in a free state. In this form it is easily removed by the oxygen of the air; and when this is effected, the colour immediately changes from venous to arterial.

Carbonic acid, which was originally described by Dr. Black as *fixed* air, has been considered lately as a *volatile* acid. The truth is, however, that this heavy gas possesses no power inherent in itself which enables it to rise in the atmosphere under a heavy pressure, and in direct opposition to the usual law of gravity. But this and other heavy gases possess the property of being acted on and lifted up, even by others that are much lighter than themselves. And when we bring a lighter to bear on a heavier gas, for which it possesses an attraction, the membrane which is between them, cannot, in that case, prevent the lightest gas from attracting even carbonic acid, which is by far the heaviest of them all. Such is the fact; but as neither the laws of chemistry, electricity, gravity, caloric, nor any other agent with which we are acquainted will enable us to explain the phenomena, we may infer, at least for the moment, that the effect is produced by a latent power of attraction, which enables certain agents to act with force upon each other, in some cases, even at a distance, and that, too, without producing any chemical change in any of their properties; for often there is no new chemical result, but merely a mechanical mixture of the different agents.

The evolution of animal heat, and the formation of carbonic acid, in the extreme circulation, is, probably, as much the effect of a chemical change as the evolution of heat, and the formation of carbonic acid in the process of combustion. The subsequent changes, however, which occur in the lungs, and by which the blood is purified, are not chemical, but, as I believe, the effect of this latent power of attraction, which enables the oxygen, in the first instance, to act with force on the blood; and, when this is effected, the same power enables the saline matter of the purified blood to attract a part of the atmospheric air into the circulation.

Oxygen is, perhaps, the most active of all agents, and possesses the power of attracting so many bodies, and so many of the others possess the power of attracting it, that this agent is never found by itself in a free state.

In common chemical language oxygen is said to possess an affinity for certain bodies; but, to speak more correctly, when a fixed substance, a metal, for example, attracts oxygen, and brings it to itself, the metal is then the *attracting*, and the oxygen is, in that case, the *attracted agent.* Or, when two gases are placed on opposite sides of a membrane, and the one removes the other, and brings it to itself, even through a dense texture, we may then consider the removing gas as the cause of the attraction, for it possesses a greater affinity for the other gas than the attracted gas possesses for it; and, therefore, though, in the end, there may be, in some cases, a mechanical mixture of the different gases, yet, in the first part of the process, it is the attracting gas which brings the other to itself, and, therefore, the fixed or removing gas ought to be considered, in this instance, as the attracting agent.

Carbon is one of the most fixed agents in existence, and wherever it exists, it invariably attracts oxygen to itself; and, were it not for this, there would not be a steady flame in the process of combustion; but when the carbonic acid is once formed as the result of this union, and when oxygen is brought into contact with the acid, the attraction is reversed, for the oxygen, in place of being the attracted, is now, as we have seen, in this instance, the attracting agent.

The carbon in the extreme circulation readily attracts the oxygen from the arterial blood; but when the acid, which is thus formed, and diffused in the venous current, is exposed to the air in the pulmonary organs, the acid is then rapidly removed by the oxygen, and this agent possesses so strong an attraction for carbonic acid, that the delicate intermediate membrane presents no obstacle to the immediate removal of the heavier gas.

Those who argue that the union of oxygen with carbon takes place in the lungs, believe that the carbonic acid may be formed in these organs in one or other of two different ways. In the first place it is supposed, that the carbon may leave the blood of its own accord, penetrate the membrane, and combine with the oxygen in the air-cells. This, however, is impossible; for carbon is one of the most fixed agents in the whole range of existence; and though this may attract oxygen through the membrane into itself, yet oxygen cannot attract carbon through the membrane into the cells; and as carbon is a fixed substance, and has no inherent power of penetration, it consequently cannot exhale or penetrate the membrane, to unite with the oxygen out of the blood.

It has been supposed by others, that the oxygen passes through the membrane to unite with the carbon in the circulation; and that the carbonic acid is thus formed, not in the cells, but in the blood itself. To this it may be answered, that carbonic, like all the other acids, gives a dark colour even to arterial blood, and if this acid were, in reality, formed in the lungs, not in the cells, but *in the blood itself*, then the first effect of the air would be not to brighten, but to make the blood much blacker in colour than it had been before. This, however, is exactly the reverse of what occurs in reality; for the first effect of the air on the blood is not to blacken its colour by the immediate formation of an acid in the blood itself, but instantly to brighten the whole of the dark fluid exposed to its influence, by the immediate removal of the carbonic acid, which is, in reality, the true cause of its dark appearance in the venous circulation.

We have seen that the blood in the extreme circulation is converted from arterial to venous, partly by the loss or rather by the change of form in the oxygen which it contains, and partly by the addition of carbonic acid. When this acidified or impure fluid arrives in the lungs, the first effect of the oxygen is to attract the acid from the blood which is exposed to its action; and when this is effected, the blood becomes florid from having lost that which had been the cause of its dark colour, for it is now, and exactly in proportion to the removal of this impure gas, that the saline matter, when no longer incommoded by the presence of a free acid, acts on the colouring matter with all the force of the neutral salts, and gives to the vital fluid in the lungs the same bright, arterial colour which we can produce even in the darkest dead blood out of the body, by adding to it a small quantity of the muriate or the carbonate of soda, the muriate of potass, or, in fact, any one of the other neutral salts that form an essential part of the blood in its healthy state. Or, the same thing may be stated thus:—Scarlet is the natural colour of the blood, such as is found in the arterial system, and this colour is produced by the action of the saline ingredients on the colouring matter. This arterial blood, however, is blackened in the extreme textures by the addition of a free and insoluble acid; but when this dark or acidified blood is exposed to the air in the lungs, the oxygen instantly removes the acid from the circulation, and then the blood in these organs immediately assumes that scarlet or arterial tint which is, in reality, its natural colour.

The absorption of oxygen is not, as is supposed by most writers, the first step in the process of respiration; for the first effect of the oxygen in the lungs is instantly to attract the carbonic acid and bring it to itself; but carbonic acid does not possess any attraction for pure air, and for this very reason, blood that is saturated with this acid cannot attract oxygen into the circulation. The first effect of the air is, as I have stated, to remove the acid; and when this is effected, the properties of the blood become, in an instant, completely changed. In the commencement of the process, the air is the active agent, and removes the acid from the circulation; but when this is effected, the blood then becomes the acting power, and attracts a portion of atmospheric air, which contains oxygen combined with nitrogen, equal in quantity to that which had been used in the extreme circulation. The pure air immediately replaces the cause of the impurity (*viz.*, the carbonic acid which has just been removed), and, in this way, the blood is converted from venous to arterial, partly by the loss of carbonic acid, and partly by the subsequent addition of atmospheric air, the oxygen of which is now taken into the circulation, probably almost entirely for the express purpose of combining with the free carbon, in the extreme texture and evolving heat, where the blood is again converted from arterial to venous.

At the high temperature of 98°, the free carbonic acid of the venous blood is rapidly removed by the external air. This effect, however, is also facilitated by the peculiar structure of the pulmonary organs; and when we recollect the immense extent of surface through which the blood is exposed to the action of the oxygen in the innumerable air-cells, the incessant renewal of the air in the lungs, together with the extreme thinness of the porous intervening membrane, and when we consider also the powerful attraction which oxygen possesses for the carbonic acid gas, we shall then not be surprised at the great rapidity with which the oxygen removes the impure gas from the blood in the lungs, or, when this is effected, the force with which the blood attracts the air into the circulation.

The carbonic acid is removed by the oxygen; but the air is attracted into the circulation, probably by the saline matter of the blood. While in the system, the greater part of it appears to be insoluble, and exists in the arterial circulation in a free state [*], until it arrives at the extreme vessels throughout the body, where the arterial circulation ends and the venous begins. It is there that the oxygen changes its form, and, in proportion as the blood circulates through the extreme texture, a part of this gas is constantly uniting with the fixed carbon for the purpose of maintaining the high heat of those animals that have warm blood.

When the carbon attracts the oxygen in the extreme texture, there is no increase of volume; and when the acidified blood arrives in the pulmonary organs, the acid is first removed, while this is immediately replaced by the pure air which is attracted into the circulation, and this exactly compensates in volume for the loss of the carbonic acid; for both in health and disease the quantity of oxygen taken into the circulation compensates exactly for the quantity of acid which is removed in the pulmonary organs; and the quantity of this depends almost entirely on the chemical changes, which occur when the blood passes through the extreme vessels.

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[*] The evidence in favour of this will be afterwards given.

The following experiment was made in a room, where the temperature of the air was at 64° Fahr.

A rabbit, apparently about six months old, was killed, by completely destroying the structure of the brain with a sharp bodkin. The chest was opened about fifteen minutes after death. The lungs collapsed as usual on exposure to the air. Both sides of the heart appeared to be gorged with black blood; the coronary vessels were beautifully injected; but on its first exposure to the air, there was not the slightest motion in any of the fibres; and the heart appeared to be completely dead. The pericardium was removed, and in about two minutes after, one of the superficial branches of the coronary arteries, on the right ventricle, commenced to contract, and acted with force until it had propelled the whole of its contents into the substance of the ventricle; the artery then ceased to act, but only when it was empty of blood. About this period the right auricle showed signs of life, and commenced to contract in about three minutes after it had been exposed to the external air. Its action, at first, was very feeble, but it gradually began to act with more force. The lungs, the liver, &c., became more red. The blood in the right auricle soon became florid; and the auricle itself resembled, in colour, almost exactly a scarlet bean. The ventricle was almost entirely without motion, but the right auricle continued to contract from below upwards for two hours and forty-five minutes after the action had commenced, in consequence of exposure even to the cold atmospheric air, and three hours after the structure of the brain had been entirely destroyed.

Now, from the above experiment, it appears that the heart ceases to act soon after death, that is, so long as it is supplied only with impure blood; but when the chest is immediately opened, and the heart, the large vessels, the lungs, &c., are exposed to the air, the acid is removed by the external oxygen, and the blood in the right auricle is purified, at least to such a degree as to keep up the action of the auricle three hours after the structure of the brain has been completely destroyed; and if the oxygen can remove the acid and purify the blood, even through the coats of the large veins, the auricle, &c., to such a degree as to keep up the action of the heart for so long a period after the complete destruction of the brain, and that too when the organs are directly exposed to the cold air, we need not then be surprised at the rapid removal of the acid through the delicate membrane that separates the blood from the air, in the pulmonary organs, particularly when the temperature of both the blood and the air are at 98°.

It has been asserted by some of the writers on hot climates, that a high heat is peculiarly destructive to the liver, and that there is in all warm countries an increased secretion of bile in consequence of the continued high temperature to which the inhabitants are exposed. This has been a common opinion; but my conviction is that it is founded on error. Messrs. Tiedeman and Gmelin, to whom, in other respects, science is so much indebted, have pressed this false fact into their service on purpose to prop up a certain theory of their own with respect to the functions of the biliary organs. These gentlemen believe that this increased secretion of bile in tropical climates compensates for a deficiency in the purification of the blood in the pulmonary organs, which is said to be caused by the rarefaction of the air in consequence of the high heat. Now, in this instance, both the assertion and the conclusion which they have drawn from it are equally incorrect; for in the West Indies, the smallest islands are decidedly the driest, and, of course, the hottest; yet in those situations where there is no stagnant water, the inhabitants are as free from disease of the liver, or any increased secretion of bile, as those that reside in any other part of the globe.

Neither is it the fact that, in hot climates, the air is rarefied to such a degree as to diminish the power of the oxygen in purifying the blood. Nature is much more perfect in all her works than such an idea would lead us to infer: for the rarefaction which might otherwise be produced by the high heat, is prevented by the heavy atmospheric pressure, which is so uniform and so great within the tropics, particularly in those low situations near to the level of the sea, where the temperature of the air is, of course, much higher than in those situations that are more elevated.

In certain localities, both in tropical and in northern countries, the liver is frequently deranged; but this derangement is produced not by heat acting directly on the human body; for it exists only in those situations where there is, at the same time, stagnant water to be acted upon by the heat. A high temperature is necessary for the generation of the marsh poison; and this poison is peculiarly destructive to the biliary organs*; but this and the increased secretion of bile is the effect of the marsh-poison, and not of any deficiency of oxygen in the air; for the blood is as freely purified in hot as in cold climates.

In cold weather we use more food, more animal heat is evolved, there is more carbonic acid formed in the extreme circulation, and more oxygen required in the pulmonary organs. A given volume of air contains more or less oxygen in proportion to its density. Cold condenses the air; and, although the volume of air which is taken into the lungs in winter may be the same as in summer, yet from the density of the atmosphere there is then more oxygen in this air than there is in the same volume when the weather is warm, for then there is less oxygen required in the pulmonary organs.

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* This diseased state of the biliary organs, which is so common in swampy districts, is not peculiar to the human species; for all the inferior animals are more or less affected by the poison. I was informed by a respectable physician in the Genesee country, that he had visited the market at Rochester almost every Sunday for seven years; but, during the whole of that period, he had scarcely seen a sound liver in any of the animals that had been slaughtered there.

A good deal has been said about the difficulty of breathing upon high mountains; I believe, however, this difficulty, if it does exist, arises partly from the increased exercise, and, perhaps, in some degree, from imagination, in consequence of a preconceived theory. I have been high enough on the Andes to have felt this difficulty had it really existed; but when I had ascended to the top of a very high mountain, after a little rest, I could breathe as freely as I had done in the morning, in a plain nearly on a level with the ocean. When we ascend, the cold increases exactly in proportion to the diminution of the atmospheric pressure; and the density in the air, produced by the cold, prevents that rarefaction in the atmosphere and difficulty of breathing which, except for this circumstance, would certainly occur. But, from the increased density produced by the cold in the higher atmosphere, it is probable that the air, even on the top of the highest mountain, contains nearly as much oxygen in the same volume, as that which exists on the surface of the sea.

In cold weather we observe that there is a quantity of moisture in the air, which is evolved from the lungs. I believe, however, that very little of this moisture is derived from the blood. It is a fact that atmospheric air parts with its moisture exactly in proportion to its heat. The cold air which is taken into the lungs is soon heated to the temperature of 98° or 100°, and the air which is most used and most heated rises to the surface. This, in expiration, is forced out from the chest, and when this heated air comes in contact with the external cold atmosphere, the moisture which it contains is immediately condensed into vapour by the cold; but the greater part of this is derived from the air which has been heated in the lungs, and not from the blood. This appearance is not observed in warm weather or in hot climates, merely because the atmosphere is not cold enough to condense the moisture in the air whch has been heated in the pulmonary organs.

As I have stated my belief in the existence of oxygen, or rather of atmospheric air* in the arterial, and of free carbonic acid in the venous circulation, it may be proper to add the result of some experiments which I have made with the solar microscope.

When we observe a drop of blood by the aid of this instrument, the globular appearance is, of course, most distinctly seen in the red fluid, when the globules are surrounded by the colouring ingredient; but that this is not the cause of the globules is evident from the fact that they are just as numerous, and nearly as distinct in the clear serum, even when it first separates, as they are in the blood, which contains colouring matter.

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* Oxygen, in its pure state, would be too stimulating for the purposes which it is intended to serve; and the nitrogen, probably, serves the same purpose in the circulation that it does in the atmosphere, that is, to dilute the oxygen.

When we agitate some red blood in serum, or any other saline fluid, and then expose one drop of this in the microscope, the whole of the globules have a red colour. But when we allow the fluid to remain at rest for a few hours, the whole of the colouring matter subsides to the bottom; and when we now examine the clear serum, the globules are found to be just as numerous, and nearly as distinct, as they had been before in the red fluid.

When we expose so small a quantity as one drop of venous blood, or of clear serum, for a short time to the air, the carbonic acid is rapidly removed, and the air is absorbed, consequently venous blood is converted into arterial before it can be used in the solar microscope, and when this is applied in the focus of the lens, the globules are so numerous that they cannot be numbered; but as a gross estimate, I may say, that one drop of serum contains not less than one thousand globules. These globules are of various sizes, and the smallest are, at least, six times smaller than the largest. When put on the glass, they appear to be flattened, dark in the centre, with a white ring round the outer edge. Some of them, however, have also an internal white ring in the centre of the dark spot; but after the serum has been exposed for a short time to the high heat in the lens, the air is driven off, and the globules disappear.

In some of the cold-blooded creatures, the globules appear to be larger than they are in the human species; the blood in them is less dense, and probably the globules may appear larger, from *being less compressed* by the surrounding fluid.

Warm-blooded animals consume a much larger quantity of air, and in these the globules are infinitely more numerous than they are in the blood of those creatures that have only one circulation.

Water possesses so strong an attraction for oxygen, that it can attract this agent from the atmospheric air, even through the medium of a dense membrane. This fact was first ascertained by Dr. Mitchel, of Philadelphia, and we know that the air obtained from rain water is found to contain about thirty per cent. of oxygen, which it probably holds in an uncombined state. With a view to ascertain whether this free air in water existed in separate particles, and presented a globular appearance, similar to that which exists in the clear serum, I exposed a drop of rain water in the focus of the celebrated lens of Goring. I then found that though the globules in the water were not quite so numerous, yet they were just as distinct, and resembled in their appearance, almostly exactly, those which I had seen in the serum. These gaseous particles, from being lighter than water, generally ascend when the drop is exposed to the hot rays of the sun, and soon disappear in this fluid as well as in the blood.

I exposed a small particle of pure colouring matter, which was found on the upper surface of an albuminous, or buffy crust, but this of itself was not globular; and we have reason to believe, that neither the fibrin nor any of the other solid ingredients have a globular form, except that which is produced by the air which they contain. Pure rain water has no solid ingredients; but still when this is exposed in the solar microscope, it exhibits a globular appearance, similar to that which exists in the serum. This appearance in the water is evidently produced by innumerable particles of free air, which by some means or other are retained in the fluid, and exist separate and distinct from each other. Now, as these globules have almost exactly the same appearances with those in the serum, and as the globules in the water are evidently produced by the innumerable particles of free air in that fluid, we may infer, at least for the present, that the globules in the blood are not produced by any of the solid ingredients, but that the same cause which produces the globular appearance in the water, is also the cause of the globules in the circulating current.

When we inject a small quantity of air slowly into a vein, it is absorbed by the blood and produces no evil effect; but when we force a large quantity of air into the same vein, so suddenly that it reaches the heart before it has time to unite with the current it then causes instant death; for nothing but blood can stimulate the vascular organs, or enable the heart to maintain its action.

When we use a drop of pure rain water in the microscope, nothing is seen, except the numberless globules floating in the fluid; but when a drop of serum is put into the focus of the lens, we also observe another and distinct species of globules which are peculiar to the serum or to the blood. They are not very numerous, are larger in size, they are more round in their shape, and much darker in their colour than the common globules. When the serum is exposed in the lens to a high heat, a gas is seen shooting out from these dark globules towards the centre of the drop forming a very distinct vascular appearance; and when we observe it attentively during the formation of these vessels, the gas in the trunk of the one vessel is often attracted into that of another, forming most beautiful anastomosing branches. When the serum has been kept for some days, these dark globules fall to the bottom of the drop, and the gas which forms the vascular appearance then shoots up from the bottom towards the centre. Should these observations be verified by future experiments, they will go far towards proving the opinion of Mr. John Hunter, that the blood possesses the power of forming its own vessels, and organising itself.

Everything that contains natural vessels circulating a fluid may be considered as organised. Vitality alone can produce this species of organisation, and should it be fairly proved that the blood possesses the power of forming its own vessels and organising itself, this alone will be a sufficient evidence of its vitality.

While the living blood is still in a fluid state, it contains the germs of organised structure; but when it ceases to circulate, and becomes solid, as in the formation of the various solid structures of the body, the water and the greater part of the saline ingredients are removed by the lymphatics, and the new solids become highly organised. Mr. Bauer observed, some years ago, a vascular appearance in the crassamentum of blood which had been drawn from the body; and Sir Everard Home believed that carbonic acid was the agent which is instrumental in forming the vessels. In my own experiments, however, the serum which was used had been previously exposed to the air, and any carbonic acid in that fluid would probably have been removed by the external oxygen, or driven off by the high heat, while the fluid was exposed in the lens to the hot rays of the sun. The venous blood loses its free carbonic acid in the lungs; consequently, the arterial blood contains atmospheric air, but not one particle of carbonic acid: and as we have reason to believe that the solids, vessels, and all are formed from the arterial blood, we may infer that carbonic acid is not the gas which gives rise to the formation of the vascular tubes. Now as the vessels are not formed by the common globules, and as they do not appear to be formed by carbonic acid, would it be going too far to suppose that the electric or galvanic fluid in the blood is the agent which shoots in towards the centre of the drop, and forms the hollow of the tubes. In the living body this fluid may attract the blood into the hollow lines which it has formed; the living blood may then stimulate the surrounding solids to contraction, and in this way the solid vessels may be formed from the blood itself. In fact, I have, by a very simple experiment, given origin to the production of a most distinct vascular appearance in the albuminous crust of inflammatory blood. This vascular appearance was, of course, most distinctly seen, when viewed by a magnifying glass, but it was, also, so obvious as to be distinctly seen, by others as well as myself, even with the naked eye.

When a drop of serum has been exposed through the medium of the lens, for a short time, to the hot rays of the sun, the common globules are driven off by the heat and soon disappear; but the crystallized salts and the vascular appearance remain distinct for many days.

If we use a drop of common water, we observe the globules rising in the drop, or, when we use a saline fluid, we see the common globules and the crystallization of the various salts; but there is no appearance of vascularity, such as we observe in the serum of the blood.

I have said, that when we expose a drop of pure rain water in the focus of the microscope, there is nothing to be seen but an immense number of common globules floating in the clear fluid; and when the water is exposed to a high heat, the air is expelled, and the globules soon disappear. But, when we expose a drop of serum in the same manner, we observe not only the common globules and the formation of vessels, but, when the serum is exposed to the direct rays of a hot sun, the crystallization of the salts which it contains is then most distinctly seen. In fact, independent of the scientific interest which we naturally feel in such experiments, I can scarcely conceive any object more beautiful in this way, than that which is exhibited when a drop of red blood or of clear serum is exposed to the rays of the sun through a powerful lens, which, like that which was used in the above experiments, magnifies the most minute objects without even the slightest deception.

The presence of globules in the serum was first discovered by Mr. Bauer, who believed that they did not naturally exist in that fluid, but were formed several days after its separation from the crassamentum. This, however, is evidently a mistake, for I found that the globules were quite as numerous, and even more distinct in the serum when it first separated, than they were after it had been exposed to the atmosphere for several days.

The use of the solar microscope may probably lead to some important information with respect both to the healthy and diseased properties of the blood; but, before any solid conclusions can be drawn from the above experiments, they must be again repeated on a larger scale, and, perhaps, with more care.

Independent, however, of all theory, the following may be considered as facts:—

First:—that globules of the same appearance with those in the serum, are also contained in rain or river water; consequently, these globules in the water cannot be formed by any solid ingredients in that fluid.

Second:—that the globules are natural to the serum, and are just as numerous in this fluid when it first separates, as they are after it has stood for many days.

Third:—that there is in the blood, in addition to the common globular appearance, a separate species of globules*, which have a dark colour, and whether this appearance be produced by some of the salts, or by the electric fluid, still when these dark globules are exposed to heat, a gas shoots out, and as the drop becomes solid, is more visible, particularly in the serum, forming a beautiful and distinct vascular appearance.

I may add that these experiments were made with the achromatic solar microscope. The lens which I used, is that of Goring, at present in the possession of Mr. New. This instrument shews most minute objects, such as the animalcula in putrid water, with a degree of accuracy which is almost incredible, and magnifies without distorting the object, even in the least.

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* I have seen these globules, also, with a common microscope.

ON THE LATENT POWER OF ATTRACTION.

I have stated that oxygen lifts up carbonic acid, diffuses it equally in the general atmosphere, and retains it there, in direct opposition to the laws of gravity. We have seen the effect of this power in the process of respiration: and whatever this agent may be which causes the lifting up of the carbonic acid, its effects are of vital importance to the physiologist; for a knowledge of this will probably enable him to explain many phenomena which cannot be accounted for either by chemistry, the brain, the nerves, or the vital principle.

We all know that when blood is drawn from a vein and exposed to the air, it becomes more red, at least on the surface; but, still, this change is evidently not the effect of either a chemical or a vital process, but is produced by the agency of a power which enables the oxygen of the air to remove the carbonic acid rapidly from the blood. Temperature, however, has a considerable influence over the agency of this power: its effects are accelerated by heat and retarded by cold. When we bleed, for example, in a very warm room, the carbonic acid is immediately removed from the venous blood, and its colour becomes almost instantly more florid; but, when we bleed in a very cold atmosphere, the acid is slowly removed by the air, and the blood remains nearly as dark as it had been when it first flowed from the vein. It is for this reason that the blood of healthy persons, in hot climates, appears to be redder than in cold countries.

The temperature of the blood of the human body, when it first flows from the system, is, generally speaking, about 98°. When the atmosphere is warm, the first effect of the air is immediately to remove the acid, and the colour changes from a dark Modena red to a bright scarlet; but when the blood becomes cold, and after it has stood for days, it then undergoes chemical changes, and, under such circumstances, even arterial blood becomes dark. At a low temperature, the blood may attract carbonic acid from the atmosphere; or what is still more probable, the carbon of the blood may now be liberated in a free state, and as this is a fixed substance, it may slowly attract oxygen, either from the blood itself, or from the air, and then it becomes black in consequence of the formation of carbonic acid within itself; for this gas is not now removed from the cold blood by common air, at a time when the temperature of both is much lower than 98°.

In one of my experiments, which was made in 1827, I allowed some blood to stand exposed to the air about four days, and when the crassamentum had become dark on the surface, I then had recourse to the following means, with a view to ascertain whether air more impregnated with oxygen would or would not be able to attract the carbonic acid from the crassamentum, which was still covered with a thick layer of dense serum.

Some coarse brown paper was steeped in a strong solution of nitre, and, after this had been well dried, by exposure to the hot rays of the sun, a piece of this paper, about the size of a dollar, was ignited over the serum, and, as I expected, the acid was removed, and that part of the clot, immediately under the spot where, as I believed, the free oxygen was disengaged, instantly became red, while all the rest of the crassamentum remained as black as it had been before. This experiment, however, will only succeed when the dark solid clot is covered either with a layer of serum, or some other fluid that contains saline matter, for when we cover it with a layer of distilled water, the oxygen may or may not remove the carbonic acid; but, under these circumstances, the colour remains as dark as before.

The reddening of the blood, in the above experiment, proved that an atmosphere mixed with a larger portion of oxygen gas, can remove carbonic acid, even through a thick layer of serum; and the experiment was so often repeated that there could not be any mistake about the result. I admit that there may be some error in the explanation which I have given respecting the change of colour; but from this and some other facts which I ascertained about the same time, I was led to believe that though there could not be any new chemical affinity between oxygen and carbonic acid, yet there could not be any question of the fact, that oxygen, though a lighter gas, possesses some inherent power within itself, which enables it, not only to bring to itself the heavier gas, but to lift it up, and hold it in a state of permanent suspension in the whole atmosphere. After having witnessed the effects of this power in oxygen, which enabled it to remove the acid, even through a thick layer of dense serum, I was convinced that the carbonic acid in the lungs is not merely *exhaled*, but *attracted* by the air through the intervening membrane; for if the one can remove the other through two inches of dense serum, there is little doubt that the thin *, porous, pulmonary membrane would present no obstacle, either to the action of the air on the acid in the blood, or to the subsequent action of the blood on the air.

The above experiment, as well as the conclusions which I had drawn from it, were distinctly stated in the manuscript of a work on the fevers of the West Indies, which was nearly ready for the press, when an unforeseen circumstance obliged me, in the month of May, 1830, to leave this metropolis on a sudden and unexpected voyage to the United States of America. The original manuscript, however, which is still in my possession, had been read, before I left London, by Mr. Travers, and one or two others of my friends in this country. One of the gentlemen who had seen this manuscript approved of the rest, but advised me strongly to leave out that part of it about oxygen attracting carbonic acid; for, as he said, it was so completely in opposition to all chemistry, that it would decidedly injure the rest of the work. This gentleman accounted for the phenomenon on a different principle: his explanation, however, to me was not satisfactory, and facts are not to be knocked down, merely because the chemist cannot explain them on any yet known chemical law. But the truth is that this is not a chemical change, and whatever the real nature of this agent may be, the fact that oxygen possesses the power of lifting up the heavier gas cannot be disputed, and many subsequent experiments have induced me to believe that the opinion which I had originally formed is probably correct.

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* According to Hales, this membrane is scarcely one thousandth part of an inch in thickness.

I have since ascertained, that some of the effects of this power had been discovered *in liquids* by M. Dutrochet; but this gentleman does not seem to be at all aware of its existence in gases. Mr. Dalton, however, had discovered that hydrogen could lift up carbonic acid: but neither of these gentlemen appear to have had any idea of the existence of an independent power acting as the cause of the phenomena which they had observed. Dutrochet believed that rarer fluids possess the property of passing through a membrane to unite with those that are more dense. This, however, is evidently an error, for there are many exceptions to this rule, even in liquids; and, in gases, those that are lightest, and, of course, least dense, generally bring to themselves those that are most heavy. Oxygen, for example, which is less dense than carbonic acid, possesses the power of removing the denser and heavier gas, even through the medium of a membrane, lifts it up in opposition to its greater gravity, and by the same power holds it equally diffused and permanently suspended in the whole atmosphere. In fact, so far is the cause assigned by Dutrochet from being correct, that rarer fluids often attract to themselves those that are more dense, even in preference to those that are much rarer. When we make, for example, a mixture of water and alcohol, put this mixture into a bladder, and suspend it in the atmosphere, the oxygen, or something else in the air, removes the water, which is the denser fluid of the two, while it leaves the less dense spirit in the bladder by itself, and in this way we can obtain a pure spirit of a stronger proof than we can by distillation. The air, also, though more rare, attracts the denser fluid of perspiration from the skin, but the force of the attraction is modified by the quantity of moisture which the atmosphere already possesses. In hot, but dry, weather, the perspired fluid is rapidly removed from the skin, cold is produced by this process, and though the thermometer be higher, we feel the heat much less oppressive than we do in less hot, but in moist weather.

As a general rule, and speaking relatively, the atmosphere contains more condensed moisture in cold countries than in hot climates: and for this reason, the heat is often much more oppressive in England at 78°, than it is in the West Indies when the thermometer is, perhaps, more than ten degrees higher than this.

Volume, or quantity, has also an influence over this power. When a small quantity of water is inclosed in a bladder, in a free circulation of air, the oxygen is then the attracting agent, and the water is removed by the less dense fluid, through the medium of the membrane, and diffused in the air. But when we tie a membrane over a tumbler, and immerse this in a large volume of water, the water is then the attracting agent, and the oxygen is removed by this into the denser fluid, leaving the nitrogen in the tumbler in a pure state.

There is at this moment, in France, a disposition amongst the French physiologists to make the same use of the electric fluid which some in this country frequently make of the living principle, or the nervous influence; that is, to account for all difficulties which they cannot otherwise explain. Dutrochet not only believed that density was the cause of endosmose, but he believed, also, that electricity was the power which enabled the rarer fluids to enter the more dense. This, however, is evidently incorrect: for endosmose and exosmose, as he calls them, are not, as we have seen, the effect of density; and the electric fluid can neither remove carbonic acid, through a membrane, nor purify the blood in the pulmonary organs. It is, therefore, more probable that this effect is produced both in gases and liquids by some latent power of attraction, which, in certain instances, enables those fluids that are within a membrane, to attract to themselves those that are without; but this power is so far from being the effect of either electricity or chemical affinity, that it is, probably, the true cause of the electric, the magnetic, the heterogeneous, the cohesive, the capillary, and, in fact, of almost every other species of attraction.

It does not appear to be the smaller quantity of electric fluid in bodies that attracts the greater; for agents, in general, do not seem to possess any decided power of attracting themselves. It is, therefore, more probable, that the want of electric matter is the cause of the negative state, and this latent power of attraction, existing in certain bodies, enables those that contain less of the electric matter, to attract to themselves a part of the electric fluid from those that contain it in a greater excess. When a cloud that contains less of the electric fluid, comes within the striking distance of one that contains it in excess, it is not, in this case, the smaller quantity of electric matter that attracts the greater, but, probably, this power of attraction in the negative cloud, which enables it suddenly to attract the excess of this subtile fluid from the one that contained it in the greatest proportion.

It is possibly not the agents themselves, but the existence of this power, inherent in certain bodies, which enables them to act with certainty, and, in some cases, with great force upon one another. The effects, however, as well as the real nature of this power, are, as yet, only beginning to be understood; but the evidence in favour of the existence of this attraction is quite as strong, if not stronger than the proofs in favour of the existence of either gravity or caloric. Gravity may be the effect of this power, assisted by the force of atmospheric pressure; for when this is removed, a guinea does not fall any faster than a feather. But by this latent cause of attraction gravity is often deprived of its power; for, in certain cases, it acts in opposition to gravity, and enables agents that are light to lift up others that are much heavier than themselves, and by the same power they are equally diffused, and held in a state of permanent suspension.

There is something mysterious in the composition of the atmosphere, which cannot be accounted for by any theory that is merely chemical; and still less by the doctrine of gravity. When its three gases are combined so as to form the atmospheric air, gravity may then keep these gases as a whole near to the surface of the earth; but neither gravity, chemistry, nor electricity, will enable us to explain why the lighter gases lift up those that are heavier, and hold them in a state of permanent suspension, even in the air of the highest atmosphere that has yet been examined.

Nitrogen, as is well known, is the lightest of the three atmospheric gases; but this agent possesses a sufficient attraction for the oxygen, so as to hold it in a state of suspension; and by means of the same power, the oxygen attracts the heavier carbonic acid, lifts it up, and, as I have before stated, holds it suspended, even in opposition to its greater gravity; and, but for the existence of this power, the acid in the atmosphere would never rise. We know, however, that it does ascend; and were it not for the effect of this attraction in holding it suspended, the whole of that heavy gas would, from its greater gravity, fall down, and occupy the lowest, or that most important part of the atmosphere which is immediately over the surface of the earth, so as to render it quite uninhabitable. The oxygen would, also, if left to itself, remain in the centre, while the nitrogen, from being the lightest, would naturally ascend, and occupy the highest stratum of all; but as neither of these gases individually can support life, they are, by means of this power, and, for a wise purpose, kept constantly united in a given proportion, and, though in a state of mere mechanical mixture, still the heaviest does not fall, and the lightest does not rise, at least, by themselves in a free state.

Though carbonic acid is by far the heaviest of all the gases, yet, in point of fact, the upper strata of the atmospheric air contain more of this gas than that portion which is nearest to the earth. This has been proved by the late experiments of Saussure. It is probable, however, that this is only the case in those places where there is extensive vegetation. The leaves of vegetables attract carbonic acid from the lower air; this is decomposed in the circulation of the plant; the carbon is retained for the formation of the woody part, and the oxygen is removed in a free state by the nitrogen of the atmosphere.

This species of attraction has been called a new power. It is evident, however, that this cause, whatever it may be, must have been in existence even from the commencement of time. Some of its effects have been observed by Priestley, Dalton, Dutrochet, &c.; but the existence of a separate species of attraction, acting independently of electricity, or of any chemical affinity, as the remote cause of the phenomena, has scarcely, I believe, until very lately, been even suspected. This power is, probably, the cause, but it is not the effect of chemical affinity; for, in some instances, it acts at a distance, and in others there is merely a mechanical mixture of the different agents, without any new chemical combination. It is this, probably, which enables water to hold salts in solution, for there is no chemical affinity nor any new chemical combination between the water and the saline ingredient. It is this also which enables the atmospheric air to attract suddenly and with force the aërial poisons which produce fever, certain odours, &c. &c., to windward, and to a considerable distance from the spot where they originally emanate. In fact, such is the force of this power, that even metallic particles are, in some cases, attracted by the air, and held for a time in the atmosphere, in a state of complete suspension. It is well known, for example, that healthy persons have been affected with mercury, merely by sitting for a few hours near to those who were in a state of salivation.

This power acts more or less in any degree of temperature; but in general its effects are, as I have said, accelerated by heat and retarded by cold. Its agency, as I have also stated, is essential to life; for to this we are indebted for the wholesome combination of the atmospheric air, as well as the purification of our blood. The cause of these phenomena is in constant operation, and its effects are as obvious as they are important: but, as yet, the very existence of this cause, like that of gravity or heat is known only by its effects; but these effects are quite as obvious and fully as important as those of either gravity, caloric, or the electric fluid.

A carbonic atmosphere can only exist where there is some local cause for the rapid evolution of that gas; and even then it is only met with in those places where the acid is evolved much faster than it can be removed by the oxygen of the air. In cold countries, for example, where the attraction goes on more slowly, the carbonic acid generally occupies a considerable space immediately over the liquor in the fermenting vats: but, in the West Indies, the acid is removed so rapidly by the air, that candles burn with a bright flame even down to the very surface of the fermenting fluid. I have often ascertained this fact in the butts that are used for fermenting the cane-liquor before it is distilled into rum; for even when the liquor was two feet below the upper edge of the air-tight butts, and when the fermentation was at its height, still, during the heat of the day, the acid was so rapidly removed, that animals could live in that space, and a candle would burn immediately above the fluid as well and as bright as it had done in the open air.

During my last residence in the United States, I visited the Saratoga springs, near the Hudson river, where I remained about five weeks; and whilst there, as I had both time and an excellent opportunity, I not only repeated some of my former experiments, but made a number of others, with the natural carbonic acid of the High Rock. The greater part of these experiments may, perhaps, be published hereafter. In the mean time, I may state that they completely confirmed the results of my former investigations, and strengthened the conclusions which I had drawn from them.

The High Rock is near to the village of Saratoga, and stands by itself in a low valley, which appears at that place as if it had been at some former period the bed of a considerable river. In this valley a stream of fresh water runs on the surface; but underneath this there exists another current of water, which is rich in saline matter, similar in quality to that which is found in the serum of the blood. This water also contains a larger proportion of free carbonic acid than, perhaps, any other natural saline water that has yet been discovered. A current of this fluid rises at some height in the cavity of the High Rock; the solid circular wall of which has evidently been formed from the solid ingredients in the saline water. The rock rises in the air in the shape of a large cone. Its lowest circle is said to be about nine feet in diameter; but this part has sunk about four feet under the surface of the surrounding earth, while the diameter of the rock, which is so large at the bottom, becomes gradually less in proportion as it ascends in the air, about five feet higher than the level of the neighbouring ground.

This rock is hollow in the centre, the internal cavity is large at the bottom, but gradually becomes less in size in proportion as it rises in the air, leaving the circular solid wall of the cavity nearly of an equal thickness from the bottom to the top, where the air within the rock communicates with the external atmosphere, by means of a circular aperture that opens upwards with a diameter of not more than twelve inches.

It is very evident that, at some former period, the water in this cavity must have risen to the top, escaped by the hole in the upper circle of the cone, and descended by its sides. But, probably, in proportion as the aperture became too small to admit of the escape of the whole body of the water, a part of the fluid has forced its way underneath by some new channel. From this, or some other cause, the fluid, at present, escapes beneath, almost as fast as it enters; for the water does not now rise more than six feet in the rock; that is, about two feet above the level of the surface of the earth; so that the surface of the fluid in the cavity is about three feet from the upper aperture.

The stream in the rock is constantly renewed; and as this saline fluid is supersaturated with carbonic acid, the constant escape of this gas produces an incessant simmering on the surface, and, frequently, is forced through the water in such quantities, and with such force, as to give to the whole body of the liquid the appearance of a strong ebullition.

As the circular wall is perfectly air-tight all round, this constant supply of carbonic acid, which is forced up incessantly from the water, can only make its escape from the hollow cavity of the rock by the aperture in the upper part of the cone. Now as carbonic acid is so much heavier than atmospheric air, we would naturally conclude, that the whole of that space in the cavity, which extends from the surface of the water to the upper opening in the cone, would be occupied with carbonic acid. This, however, is not the case, for when I put down a burning candle, at different periods, into the cavity, I found that the carbonic atmosphere seldom rose higher than from one foot to one and a half above the surface of the water, while the candle burned perfectly bright in the air, in that part of the cone which extends from the orifice to the carbonic atmosphere; that is, from about one foot, to eighteen inches, beneath the circular opening.

As the solid walls of the cone are perfectly air-tight, it is very evident from the above circumstance, that either the air, or some power which the air possesses, lifts up the heavier carbonic acid out of the cavity almost as fast as it escapes from the water. This power is in constant operation, but it acts with most force when the temperature is high, as it was in August and September, when these observations were made. I found, however, even at that season, the carbonic atmosphere rose much higher at night than during the day; and one night, when the weather was cold for that season of the year, the carbonic atmosphere had risen to within six inches of the upper orifice.

The degree of motion in the external air has also a considerable influence over the quantity of acid which accumulates in the rock. When the external atmosphere is in free circulation, and, of course, a constant supply of oxygen over the opening, the removal of the acid goes on much faster than it does when the air is still. One forenoon, when there was a breeze in the valley, I found only three inches of carbonic atmosphere over the water; on another occasion, when the air was almost still, I found that it had risen two feet and a half. But whatever it may do in winter, it never, in summer, rises entirely to the opening.

This circumstance of the rising and falling of the carbonic atmosphere in the cavity of the rock, accounts for a fact, which had been long known to the boys who are in the habit of showing experiments with the gas to strangers who generally visit that place, either to drink the water or from curiosity. When rabbits, or other small animals, are put into the aperture, and let down into the cavity about the length of the arm, they are generally killed in about three minutes; but, at other times, they can remain there for half an hour with the most perfect impunity; or, as one of the boys stated the case, 'sometimes the animals would die fast enough; but, at other times, they were so obstinate, that they would not die merely to please the people.'

To ascertain whether it was or was not the oxygen, or some power in this gas which removed and prevented the rising of the carbonic acid to the upper aperture, I had a large funnel made on purpose by a tin-smith in the village of Saratoga. This was constructed of such a size, that the large end of it exactly covered the aperture at the top. This funnel was applied to the orifice, and the part which rested on the rock was made perfectly air-tight all round with a cement of moist clay. When this was perfectly adjusted, I applied my finger to the small aperture of the funnel, to prevent any communication with the external air, and raised it for a moment, from time to time, in order to allow the common air, already in the cavity, to be forced up by the pressure from beneath. When I felt convinced that the whole of the atmospheric air had been forced out, I then fixed a moist empty bladder on the small end of the funnel; this filled slowly, evidently from the rapidity with which the carbonic gas was removed by the external air through the coats of the moist bladder. In about twenty minutes, however, it was nearly full. It was then taken off, and a small quantity of the gas contained in the bladder was passed through lime-water; and from the quantity of carbonate of lime that was suddenly formed, it was very evident that the air in the bladder was pure carbonic acid, and that this, as soon as it had ceased to be removed by the oxygen of the external air, had ascended in the cavity of the rock, not merely to the aperture, but that it had then been forced up by the pressure from beneath through the funnel into the bladder.

The following experiments, made with the natural carbonic acid, prove that membranes, much denser than that which separates the air from the blood in the lungs, will not prevent the oxygen from acting on the carbonic acid, and lifting it up through the medium of dense membranes.

A piece of moist bladder was tied firmly over a tumbler of carbonic acid, and when this was exposed to the air, a part of the acid was evidently removed by the oxygen, faster than the air could enter, for the volume of the acid in the tumbler was soon diminished, and the membrane became concave from the atmospheric pressure. But when I reversed this experiment, and tied a piece of white leather (such as apothecaries use for blisters) over a tumbler of common air, and then immersed it in the carbonic atmosphere of the High Rock, there was soon such an increase of volume in the air contained in the tumbler, that the membrane swelled up, and became so very tense, that I was obliged to remove it from the carbonic atmosphere almost immediately, for fear that the glass might burst in my hand.

In the first experiment, the decrease of volume in the air contained in the tumbler was not produced by any decrease of temperature; neither did the increase of volume in the second arise from any increase of heat; and to ascertain whether the oxygen, or something else in the air contained in the tumbler, had, or had not, drawn into itself, and that too with force, a part of the carbonic acid, even through the dense membrane, this, when it became tense, was immediately punctured with the point of a lancet, and a part of the air was drawn up from the bottom of the glass by an ear-syringe, with a long pipe. This air was passed through lime-water, which I had ready for the purpose; and a small portion of the contained gas instantly whitened the whole of the water. From this it was evident that oxygen, or something else in the atmospheric air, possesses the power of attracting carbonic acid to itself, even through a membrane much denser than that in the lungs, which separates the air from the blood, or the blood from the air.

In the above experiments, the oxygen was evidently the agent which acted on the acid, for nitrogen possesses no power of attracting carbonic acid; neither was it the carbonic acid of the air which attracted the acid from the tumbler; for one gas does not attract another of the same kind; consequently, carbonic acid can have no power in attracting itself.

It is also evidently the oxygen which removes the acid from the blood in the process of respiration; for animals cannot live in an atmosphere of either nitrogen or carbonic acid; but besides this, when the quantity of oxygen is considerably increased, the acid is removed in the lungs with great rapidity, and when the blood is healthy, and contains its full proportion of saline matter, there is more oxygen than usual attracted into the circulation, the blood becomes too stimulating for the vascular solids, it then circulates with great force, and becomes arterial even in the veins. On the other hand, when we diminish the quantity of oxygen in common air, the cause of the impurity is not attracted; the acid acts as a poison in the left side of the heart, &c., the animal breathes with difficulty, its blood is not purified, and it soon dies.

In a conversation which I had with Dr. Edwards on the subject of respiration, during my last visit to Paris, in October, 1829, Dr. E., like most others at that period, refused almost to listen to the idea of the carbonic acid being attracted by oxygen.

He told me, that he had adopted Spallanzani's opinion, and had proved by experiments, that the acid was merely exhaled through the pulmonary membrane. He then related his celebrated experiments, by which he had proved, as he said, that this was the case. I had, at that time, just returned from the West Indies, and had never until then heard of these experiments; but from the high and deserved reputation of Dr. Edwards, I could not, for a moment, doubt that the facts were as he had stated. I was, however, completely at a loss, at that time, to account for the results which he had obtained. But, soon after my arrival in London, on looking over some of Mr. Dalton's experiments, the secret of Dr. Edwards's exhalation was easily explained: for, from these experiments, it was evident that hydrogen, as well as oxygen, possesses the power of lifting up carbonic acid. And although the former possesses this property in a much inferior degree to the latter, yet it does lift carbonic acid, at least in a confined atmosphere; and this power, in the experiments of Dr. Edwards, enabled the hydrogen to remove the carbonic acid from the blood in the lungs of the cold-blooded animals which he had used. And, with respect to the young animals of the mammalia class, it is probable that they were enabled to live in the hydrogen, partly for the same reason with the others; and were enabled, like them, to evolve heat and to generate carbonic acid in the extreme vessels, from the quantity of oxygen already in the arterial circulation, which they had previously derived from the external air, or, perhaps, in part, from the maternal blood.

Mr. Dalton accounted for the phenomena, on the supposition that the particles of a gas are repulsive to particles of its own kind, but not to particles of a different gas. If this assumption be true, then as a consequence it follows, according to his theory, that one gas acts as a vacuum to another gas of a different kind. Carbonic acid, however, it appears, has no vacuum for oxygen; for this gas will not pass through a membrane to mix with the acid in a tumbler, or to unite with the blood in the lungs, so long as it contains this impure air; neither has nitrogen any vacuum for carbonic acid; for nitrogen cannot remove this gas from a tumbler, or purify the blood in the pulmonary organs. When hydrogen, however, is enclosed in a tube, it is possible that there may be a vacuum betwixt its particles, and when carbonic acid is allowed to communicate with a lighter gas in the same tube, it may rise up so as to occupy the empty spaces: but there is no proof that any such vacuum exists in any of the gases. It is, therefore, more probable that hydrogen attracts the acid, or when the lighter gas is excluded from the external air, and when a communication is made betwixt this and the acid beneath, the lesser pressure of the hydrogen will enable the carbonic gas to assume more readily its elastic form, and in this way it may arise so as to produce a mixture of the two.

Hydrogen, that is so much lighter than atmospheric air, may rise in the atmosphere from its own elasticity, or, more probably, it may be forced up by the surrounding pressure; but carbonic acid, or any other gas that is heavier than common air, can only penetrate a membrane, and rise up in this or any other lighter fluid, when there is some agent on the opposite side, to attract or to lift it up. Now, as carbonic acid can only exhale or penetrate through the membrane in the pulmonary organs, when there is oxygen, hydrogen, or some other agent in the cells of the lungs to attract it through; and as this is the fact, it still remains to be proved, that this gas, which is so much heavier than atmospheric air, while under the influence of atmospheric pressure, possesses any power *inherent within itself*, which enables it to exhale or penetrate through the membrane of the lungs, from its own elasticity, or rise in the air and hold itself equally suspended even in the highest atmosphere that has yet been examined.

It is very evident that if the carbonic acid was merely exhaled, or if it possessed any inherent property within itself which enabled it of its own accord, to penetrate through the membrane of the lungs, then nitrogen, or any other gas which does not possess any noxious quality, would be able to distend the cells, and serve just as well as common air in the process of respiration. But this, we know, is not the case; for an animal almost instantly dies, when we force it to breathe in an atmosphere of nitrogen, or any other gas that does not possess the power of removing the acid from the blood; and this is merely because there is no attracting gas present in the air cells to remove the cause of the impurity, and the animal soon ceases to live; for the impure blood passes on, without force, to the left side of the heart, without being purified in the pulmonary organs.

When cold-blooded animals are forced to breathe in an enclosed atmosphere of nitrogen, a quantity of carbonic acid may be found in the expired air. A small part of this is probably attracted by the quantity of oxygen previously in the lungs, or perhaps in the nitrogen itself, but, above all, by the diminution of the pressure in the enclosed air; yet the rapidity with which all warm-blooded animals are destroyed, in an atmosphere of PURE nitrogen, proves how incapable this gas is of removing the acid through the thin membrane which intervenes betwixt the nitrogen and the impure blood.

The effects of gases on each other have been explained lately on the principle of penetration; but a heavy gas, like carbonic acid, does not possess any such property as penetrativeness *. When we tie a membrane over a tumbler of carbonic acid, and then immerse it in an atmosphere of nitrogen, the acid does not then penetrate through the membrane, of its own accord, but remains where it

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* See Dr. Mitchell's paper, *On the Penetrativenesss of Fluids*, in the American Journal of the Medical Sciences, for November, 1830.

is, quietly in the glass. Were we even to admit the opinion of Mr. Dalton, and suppose that a vacuum in the lighter gas is the cause of the rising of the heavier air, still it is not the acid which penetrates the membrane, of its own accord. It is the vacuum which is the cause of the penetration; for the acid does not pass through where there is no vacuum, neither will it penetrate, except where there is some attracting power on the opposite side of the membrane, acting as the cause of its passing through.

But whatever there may be in an enclosed gas, yet it is very clear that there cannot be any vacuum of this kind in the general atmosphere, and as oxygen possesses the power of removing the acid in the open air, and bringing it to itself, even under the heaviest atmospheric pressure, and that too through the medium of a dense membrane, and as we have seen that such is the fact, we must, therefore, look to some other power, for neither a vacuum, nor any other mechanical law, can account for the rising up of the carbonic acid. We may therefore infer that the power which oxygen possesses of lifting up the heavier gas in the open air, cannot be the effect either of a vacuum, chemical affinity, or penetrativeness, but more probably the effect is produced by a species of latent attraction, which enables the oxygen to lift up a heavier gas, even through the medium of a dense membrane.

The experiments which prove the action of gases on each other, have lately excited an intense degree of interest amongst our professional brethren in the

United States. I say, lately, for it was only in the month of November, 1830, that two papers appeared there, for the first time, on this subject:—the one is on the endosmose and exosmose of gases, said to be written by a Dr. Faust; the other is a paper on the penetrativeness of fluids, by Dr. Mitchell, lecturer on medical chemistry, in the Philadelphia Medical Institute *. Dr. Faust's paper is dated on the 23rd of August, and Dr. Mitchell's, on the 18th of September, 1830. But their first experiment appears to have been made on the 21st of July; that is, a short time after I had mentioned the leading and by far the most important fact of the whole, not merely to one but to several individuals in that country.

Dr. Mitchell, who is the most prominent of the two writers, states that he got the first hint on this subject from a balloon, as it appears by his own statement, in 1829. He had afterwards, it seems, read an account of Mr. Graham's experiments; but notwithstanding the previous hint from the balloon, the strong facts stated in Mr. Graham's paper appear to have made no impression on Dr. Mitchell, for he says, ‘A careful perusal of Mr. Graham's ‘notice, will excite in every one who knows the ‘value of experimental interrogation, an expression ‘of surprise, at the failure on the part of that intelli‘gent and ingenious chemist, to pursue, in the only ‘true spirit of science, the investigation of a prin-

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* See the ‘American Journal of the Medical Sciences,’ for November, 1830.

ciple, one of the most striking manifestations of which had thus been placed conspicuously before him. Content with a single additional experiment, he comes, *in the ancient method,* to immediate conjectural explanation, and has thus lost an easy opportunity of making a beautiful and, perhaps, extensively useful discovery. Made at an earlier period, his observation was published in the Journal for 1829, and has since attracted, apparently, no scientific attention. Such is, usually, the fate of the most pregnant facts, which are not perceived to bear on some *generality. This one passed from my mind,* along with all the other isolated phenomena of that number of the Journal, and only shone importantly when illuminated by the *reflected light* of an extensive principle, *subsequently* developed. These remarks are made, not to throw any discredit on the character of the accomplished gentleman to whom they refer, but to correct the baneful error of ancient dogmatism, which yet weighs so heavily on the cause of nature and truth. It was true that the carbonic acid entered a closed bladder, and that, too, *with power ;* and it was equally true, that oxygen had done the same thing, in the experiment of Priestley, and that, in his hands, even common air had penetrated to replace hydrogen in a similar viscus, and yet he ascribed the phenomenon observed by him, to the *capillaries,* and the conducting power of aqueous canals.'

We may infer from this, that it was neither the balloon in 1829, nor Mr. Graham's paper, but *a reflected light,* which had induced Dr. Mitchell to commence his labours so suddenly on the penetrativeness of gases. And this is probably true; for I had, previously to the commencement of his experiments, stated to an intimate friend of Dr. Mitchell's the leading and important fact, namely, the powerful attraction which oxygen possesses for carbonic acid, as well as the conclusions which I had drawn from this fact, relative to the agency of oxygen in the purification of the blood, by attracting the carbonic acid in the lungs, through the medium of the intervening membrane. The conversation I had with this gentleman was in Philadelphia, on or about the 15th July, 1830; and when I add that the gentleman to whom I had mentioned this important fact was afterwards, to my certain knowledge, present with Dr. Mitchell while he was performing his experiments on this subject, the reader may then form any opinion he may think proper relative to the source of the reflected light which had induced the Philadelphia writers so suddenly to commence their experiments to ascertain whether gases could or could not attract each other, even through the medium of dense membranes.

Dr. Mitchell does not inform us at what particular period he commenced his labours on this subject; but I have good grounds for the belief that his experiments were not commenced until immediately after the above conversation. At least, a friend of his, who is better acquainted with what is going on in the literary world than, perhaps, any other individual in that country, and to whom I had, in July, mentioned the result of my own experiments, refused at that time to admit the possibility of oxygen attracting carbonic acid, either in the lungs or anywhere else. He argued, as others had done before, that as the carbon in the carbonic acid had already combined with as much oxygen as it was capable of uniting with, there could not be any further chemical union betwixt these two bodies, and, of course, no further effect between them. In answer, I stated to this gentleman, as I had done to others, that the change was not the effect of any chemical affinity, but, probably, produced by the agency of a species of attraction, with which, as yet, we were but little acquainted. This conversation, as already stated, occurred in Philadelphia, about the middle of July 1830; but on my return from Quebec, in the following November, I found that the effect of oxygen in removing carbonic acid, was now not only admitted by the same gentleman, but that the whole had already been published in Philadelphia, as an American discovery, with this difference, that they had added some additional experiments of their own, and had described the phenomena in a sort of language peculiar to themselves.

It is possible that I may be in error in considering the rising up of the carbonic acid as the effect of attraction; but it is also very evident that Dr.

Mitchell had adopted the same belief. He, however, makes use of the word *attraction* as seldom as possible; for this, probably, might have led at once to a discovery of the source of the reflected light. But still it is evident, from more than one expression in Dr. Mitchell's paper, that he was perfectly aware that *attraction*, and not *penetrativeness*, was, in reality, the cause of the phenomena; but, notwithstanding this, he makes use of the most equivocal language from beginning to end. In one part, and for a reason best known to himself, he denies attraction; in another, he admits that the phenomena *are not produced by any vis à tergo*, and that it must, therefore, be attributed to *some species of attraction;* yet, even after this, he talks to the last about penetrativeness, as if, by giving the discovery (if it be one) another name, he might make it out to be a different thing from the sort of attraction to which I had attributed the phenomena, and as if for the express purpose of misleading the reader, in some parts of the same paper, in which he admits attraction, he even attempts, as I have said, to prove that attraction is not the cause. In one place, for example, he states, 'But we can 'scarcely, after a fair estimate of the value of facts, 'see anything more in the power than that of *com- 'mon interstitial infiltration*,—a power marvellously 'great, but insusceptible of demonstrative reference, 'at present, to any known cause.'

Yet, notwithstanding this, we are told in the next paragraph,—'The amount of force having been 'shown to greatly exceed that of atmospheric 'pressure, we feel assured that the interstices are 'penetrated *not by any vis à tergo.* It must, 'therefore, be attributed to some species of *attrac-'tion,* the force of which, as shown by the condensa-'tion of some gases by charcoal, sometimes equals a 'power of forty atmospheres, or nearly six hundred 'pounds on the square inch.'

Now, if a heavy gas which is enclosed in a glass vessel with a dense membrane tied firmly over it, does not penetrate the membrane by any *vis à tergo*, but is attracted or drawn through by a lighter gas on the opposite side, it is not, in this case, either common interstitial infiltration, penetrativeness, or any inherent power in the enclosed gas, which is the cause of the rising up of the heavier fluid; for the gas that is within will not penetrate, unless there be on the opposite side of the membrane some attracting gas to lift the enclosed fluid, and bring it to itself; and if dense gases pass through without any *vis à tergo*, and if they be drawn through the membrane by some attracting agent on the opposite side, there must then be something more in this than either penetrativeness, or mere common interstitial infiltration; for if there were no other power, the heavier gases would remain undisturbed, and, at all events, the rarer gases would penetrate faster than the more dense. This, however, is exactly the reverse of what occurs in reality, for we have seen that the rarer gases almost invariably attract and bring to themselves those that are most dense, even in preference to those that are more rare.

The time is, perhaps, not far distant, when the knowledge of the existence of this power, particularly in gases, may be the means of enabling us to remove many of the difficulties which now exist, both in physiology and pathology; and, for reasons which will afterwards be given, it is equally probable that it may be the means of effecting an entire and favourable change, both in the theory and practice of medicine; but of this hereafter. It was known in Philadelphia that I intended to publish on this, as well as on other points connected with the blood; but both of these writers appear to have been very anxious to be first in the field. Some of their facts are of great value, and so far both of them are entitled to great credit; but their reasoning on these facts is, as I believe, anything but good. In truth it appears, from their anxiety to be the first in print, that they have not allowed themselves time either to correct their errors, or to draw scarcely even one conclusion from all their experiments that is really useful.

Both these gentlemen consider their discoveries as of importance, chiefly in enabling them to explain the changes which occur in the pulmonary organs. 'It is,' says Dr. Faust, 'principally to the illustration 'of the function of respiration, however, that we 'would invite the attention of our readers, who will, 'we hope, feel interested in following us through our 'application of the preceding phenomena to the 'various circumstances of that process on the 'chemistry and physiology of which philosophers are 'so much at variance. We hope to clear up some 'doubtful points, and to reconcile some previously 'conflicting authorities.' Such was their object; but how far they have succeeded in this, those who have read their papers can best judge.

I have already tried to prove that the blood owes its dark colour in the venous circulation, not to carbon, but to the presence of carbonic acid; I had also, during my residence in America, stated my belief that the blood might be blackened by the aëriform poisons, which cause fever, as well as by the loss of its saline matter. The Philadelphia writers, however, think very differently on this subject.

'Physiologists,' says Dr. Faust, 'who are chemists, 'but not enthusiastically so, will find constant use for 'their knowledge, without being led astray by it. 'But to adopt the view least favourable to our 'opinions, and to grant that, under extraordinary circumstances, some secret cause, independent of 'carbon, may blacken the blood, every sound reasoner will perceive that this will not affect the conclusion that, under ordinary circumstances, venous 'blood owes its colour to carbon.' Now, with the exception of Dr. Clanny's late analysis, which I know to be incorrect, and with which they were unacquainted, we have nothing to lead us to believe that one particle of free carbon exists in the blood; neither is it quite certain that pure carbon possesses the property of blackening its colour; for it is probable that carbon can only darken the blood when it attracts oxygen, and forms an acid, as it does in the extreme circulation; but carburetted hydrogen, and other combinations of carbon, which have no acid property, give to the venous blood a brighter colour than it had before; consequently, we cannot admit, without proof, either that pure carbon exists in the blood, or that this is the cause of the dark colour in the venous circulation.

Dr. Mitchell is equally explicit with respect to the existence of carbon, and not carbonic acid, acting as the cause of the dark colour in the venous current, and forgetting that the carbonic, like all other acids, gives a venous or black colour, even to arterial blood, he believes that the carbonic acid is formed in the lungs, not in the air-cells, but in the blood itself; and that this acid is, in reality, the true cause of the arterial colour. 'The oxygen,' he says, 'penetrates 'slowly the membraneous tissue, to infiltrate and 'brighten the blood; carbonic acid is immediately 'formed, and being a gas differing from the re'mainder of the air, yet, in the air-cells its tendency 'is to return, to *penetrate* that air, and thus escapes 'through the trachea along with it. The oxygen 'enters *because there is oxygen enough behind to 'permit that,* and it is also an observed fact. The 'carbonic acid formed makes its escape, because '*invited* by the molecular tissue of atmospheric air. 'Keeping up any reference to known facts, we can 'scarcely doubt the truth of our explanation, or venture to adopt any other.'

Now, even admitting for a moment, that carbon, and not carbonic acid, is the cause of the venous colour, it is not, it appears, the fixed carbon that attracts the oxygen into the circulation, but the oxygen, which penetrates slowly through the membrane of its own accord;—and why? merely, because there is so much oxygen left in the general atmosphere, that this small quantity can be very well spared. Another reason for its penetration is, that the oxygen is *observed* to penetrate through the membrane to unite with the carbon; but those only will observe this who are ignorant of the fact that all the acids destroy the red colour of the circulating current. According to the same theory, when the acid is thus formed in the blood itself, as he avers to brighten its colour, it is not, it seems, attracted out of the circulation by the oxygen of the air, but it comes through the membrane again, of its own accord, or rather because it receives an invitation to do so from the molecular tissue of the atmospheric air.

We are told, in another paragraph, what we all know; namely, that carbon is one of the most fixed substances in nature: consequently, he admits that as this agent does not possess any penetrative quality, it cannot, therefore, like the acid, penetrate the membrane; but the oxygen infiltrates the blood, and the whole of it is immediately converted into an acid. It is this acid that gives to the blood in the lungs its arterial colour; and then this impure gas, which according to Dr. Mitchell's theory is the cause of the arterial colour, is immediately invited to return by the molecular tissue of the atmospheric air. It is now well known, however, that the quantity of oxygen which is taken into the lungs almost invariably keeps pace with the quantity of carbonic acid which is evolved. And, may we ask, if the whole of the oxygen be immediately converted into an acid, and if this acid be, in reality, the true cause of the brightness, what is it that keeps up the scarlet appearance of the blood in the arterial system, when the acid which is the cause of the arterial colour is immediately removed from the circulation? But to leave theory and return to facts,—we know that acids do not brighten the blood; and the fixed acids not only blacken the colour, but destroy the structure of the colouring matter so irretrievably, that, after this, its healthy appearance cannot be restored, even by the addition of saline matter. The carbonic is, perhaps, the weakest of all the acids; yet when we agitate arterial blood in a phial of this gas, the colour of the whole is immediately converted from arterial to venous. We know, also, that when venous blood is drawn from the body and exposed to the air, in a high temperature, the effect of the air is not first to blacken the blood by the immediate formation of carbonic acid in the blood itself, but instantly to brighten the colour by the sudden removal of this gas from its upper surface; and, in fact, when the temperature is high, this attraction of the acid is so rapid, that a great part of it is probably removed before the blood enters the cup that receives it.

When we set fire to the end of a piece of red sealing-wax, so long as the flame continues, the fixed carbon of the melting wax attracts the oxygen of the air into itself; and that part of its substance which is liquefied and in flame becomes nearly black from the excess of acid which is formed in the wax during the combustion. This may be considered as something analogous to the process which takes place in the extreme circulation where the blood is converted from arterial to venous. But when we drop the dark-coloured fluid wax on a piece of paper, blow out the flame, and then stir it so as to expose the whole of it to the air, the acid is rapidly removed, and in proportion as this takes place the colour changes from a very dark to a bright red. And this is almost exactly what occurs when the dark acidified blood is exposed to the air in the pulmonary organs; for the oxygen rapidly removes the acid, and the colour instantly changes from a dark purple or Modena red to a bright scarlet.

We have seen that it is not in the lungs that the blood is blackened by the immediate conversion of oxygen into an acid, for this change evidently takes place long before the blood enters the pulmonary organs. When we open a vein, even in the foot, the blood is dark; or when we open the abdomen of an animal, immediately after it is killed, we observe that the blood in the mesenteric veins is nearly black; but the first effect of the air is not to darken its colour by the immediate formation of an acid; for the moment that this dark blood is exposed to the air the acid is removed, even through the coats of the vessels, and the colour almost instantly changes from venous to arterial. This would not be the case if the carbon attracted the oxygen into itself; for if the oxygen were immediately converted into an acid in the blood itself, its first effect would be, not to brighten, but to blacken the colour within the vessels; and this, as we have seen, is exactly the reverse of what occurs when the blood is exposed, either directly or indirectly, to the atmospheric air.

Dr. Mitchell admits that their theory throws no light on the doctrine of animal heat; but the fact is, that the doctrine of animal heat gives a deathblow to the very theory which they have undertaken to advocate, even in opposition to their own facts. Neither oxygen nor carbon, individually, can evolve heat; but it is a law, to which, I believe, there is no exception, that whenever, or wherever, carbon attracts oxygen, so as immediately to form carbonic acid, heat is evolved in proportion to the quantity of oxygen consumed, and, of course, to the quantity of the acid which is formed. Now, if this union takes place in the pulmonary organs, either in the cells or in the blood itself, then the lungs would be burning hot, while the extremities, at the same time, would be deadly cold.

This objection to the theory, as originally proposed by Dr. Black, was so strong, that that philosopher himself never, I believe, even attempted to remove it. Dr. Crawford, however, tried to get rid of this difficulty by one of the most brilliant efforts of human ingenuity; but, unfortunately, the very pillars on which Dr. C. has built his hypothesis have been found to be unsubstantial; and, in point of fact, it appears now that the truth is exactly the reverse of that which was stated by him; and, should it be necessary, additional facts may be brought forward to prove that the venous has, in reality, a greater capacity for heat than arterial blood. The facts, however, already before the profession, are quite sufficient to do away all faith in Dr. C.'s theory of animal heat; consequently, the original objection to Dr. Black's theory of the union of the carbon and oxygen in the pulmonary organs remains in full force against it even to this day, and no one now who asserts that these agents unite in the lungs, so as immediately to form carbonic acid either in the air-cells or in the blood itself, can expect the profession to put any faith in his hypothesis, until he is prepared to inform us why the lungs are not in a state of combustion, or in what manner the excess of heat is so suddenly conveyed from the lungs, and equally diffused all over the living body. In this attempt Dr. Crawford, with all his talents, has completely failed, and, therefore, the hypothesis of Black must still be considered as totally untenable. Dr. Mitchell and Dr. Faust, however, have both advocated the old hypothesis, but they have neither supplied us with one argument nor a single fact which can tend to remove the above objection. In the commencement of their papers, they promise to clear up doubtful points, and to reconcile the previously conflicting authorities; but this promise, most assuredly, they have not kept. I have said before, that some of their experiments are of great value; but so far from reconciling conflicting authorities, clearing up doubts, or throwing any light on the theory of respiration, they have left it as much in the dark as it had been before either of these gentlemen undertook to remove the difficulties which had previously existed.

The American writers are better known, and, perhaps, for this reason, they are more appreciated in France than in this country; but, be this as it may, there is at this moment, in America, a disposition in a certain class of practitioners to give a decided preference to every thing that emanates from the French school. The existence of this attraction in gases is, perhaps, even more important than the knowledge of its effects merely in liquids. Dr. Mitchell is evidently acquainted with the experiments of both Priestley and Dalton. He was also, probably, aware that his own claim to this discovery would soon be put aside, even by his own countrymen, for the most important of all their facts, namely, the effect of oxygen in attracting carbonic acid, was well known to many individuals in America, before either Dr. Faust or himself had written one syllable on this subject; and, therefore, with great apparent liberality on his part, Dr. M. seems anxious to make over the sole and entire merit to a French philosopher. 'On the whole,' says Dr. Mitchell, 'captivating as is the method, and elegant as are the 'experiments of this little volume of M. Dutrochet, 'it does not bring additional support to his doctrine 'of endosmosis. Yet, whatever may be the issue 'of the experimental investigation, to whose rigid 'scrutiny this most important subject is committed, 'the philosopher and physician can scarcely find 'language adequately to express the obligation, the 'high obligation, under which science has been laid 'by the elegant labours of M. H. Dutrochet. In 'him we discover the *punctum saliens* of a principle 'which is the master-spirit of animal and vegetable 'motion, the ruling power of chemical science, the 'governing influence of atmospheric composition, the 'presiding genius of respiration, circulation, and 'nutrition, the cause of disease, and the restorer 'of health. But whatever may be now his fame, 'how little is it compared to that which may be 'anticipated for him by one who takes even a 'careless view of the mighty field of novel observa-

'tion, just redeemed from the rich wilderness of 'nature!'

Now, notwithstanding all this, Dr. Mitchell himself admits, in a former part of his paper, that Dutrochet had never even dreamed of the existence of a separate power acting as the cause of endosmose or exosmose. And if the mere circumstance of having observed some of the effects of this power, without even the slightest suspicion of the existence of the cause, were to entitle Dutrochet, even for a moment, to the whole credit of the discovery, then he, in return, must resign it over, to add one more to the brilliant discoveries of Dr. Priestley, or to those of Mr. Dalton; for both of these gentlemen had observed the effects of this power long before M. Dutrochet had ever made, or thought of making, even one experiment on purpose to establish the existence of endosmose,—but with this difference, that after having observed the phenomena, they described them in the language of common sense, and without using such terms as *endosmose or exosmose, nulifiers, sedatifs de l'endosmose, penetrativeness,* and other hard words that will be altogether unnecessary when the cause of the phenomena is once understood.

It must have been known, I should think, even to Adam, that when an apple was left to itself it would fall to the earth. Newton, however, gained a part of his fame, for having been, as is believed, the first to discover the cause of its descent: and, however much the profession may be indebted to Monsieur M. H. Dutrochet, for his diligence in having ascertained some of the effects of this power in liquids, yet, even Dr. Mitchell himself has taken great pains to prove that Dutrochet was completely in error with respect to the cause.

I shall not, for the present, insist on the strong internal evidence which is contained in the two papers to which I have referred; neither would I give the value of one straw to be able to prove that these gentlemen were led to the investigation of this subject in consequence of the facts which I had previously stated to several individuals in that country, even to the editor of the very journal in which these papers were published, and that too before either of these writers had commenced their experiments. But as some of their results are similar to my own, I will afterwards take pains to prove that if there has been plagiarism, I, at all events, have not borrowed the original ideas from them. I may also, afterwards, prove that the leading facts which I have mentioned in this publication had been stated both in this country and in France, previous to my last visit to the United States. I can also prove that, long before I went to America, I had adopted the opinion that the carbonic acid was not merely exhaled in the lungs, but that the blood was purified in these organs in consequence of the sudden attraction of this gas by the oxygen of the atmospheric air. This was clearly and distinctly stated in the manuscript of my work on the fevers of the West Indies. The original manuscript I have still in my possession; it is in the handwriting of another person, and was not only copied by that individual, but read by others, before I went to the United States. It is also a fact that the leading and most important of all these experiments was well known to many individuals, even in America, long before the publication of their papers. I could bring forward many proofs that such is the fact; but this will scarcely be necessary to those who are acquainted with the high respectability of the gentlemen who are the writers of the following letters. The first is from Dr. A. Hosack, who has now almost entirely retired from practice, and living in independence in his own country. The second is from Dr. H. M'Lean, of New York, who holds at present in that city the same high rank amongst his professional brethren which Dr. Baillie held in this but a few years ago. The third is from Dr. A. Hosack, the son of a gentleman who is an honour to his country, and has done more good in the medical profession than any other individual on that side of the Atlantic.

' *New York, Jan, 5th,* 1831,

' My dear Sir,

' Your favour of yesterday causes me to ' recur with pleasure to the month of August last, ' the whole of which (as you know) I passed at the

'Saratoga springs; it was particularly agreeable to 'to me, as we were fellow lodgers.

'I recollect well your opinions on the subject 'of carbonic acid; viz., that the oxygen of the 'atmosphere possesses a strong latent attraction for 'that gas, and though there might not be any 'new chemical combination, yet there was no ques'tion of the fact that the oxygen possesses the pro'perty of removing the carbonic acid from vessels 'even where a dense membrane was interposed be'tween the oxygen and the acid. As this circum'stance was at that time entirely new and interesting 'to me, I accompanied you frequently to the High 'Rock to witness your experiments, and as they 'appeared conclusively to prove the fact, I do not 'hesitate to say, that from the daily conversations I 'have had with you on this subject, and the various 'experiments which you kindly invited me to wit'ness, I was perfectly satisfied that such an attrac'tion did exist before any of the recent publications 'on the same subject made their appearance in this 'country.

'Having been an annual visiter at these springs 'for the last seventeen years, I have heard of and 'witnessed many curious facts and phenomena (par'ticularly at the High Rock) which have not hitherto 'been explained or accounted for by any of the phi'losophers or medical men who have visited that 'place; but the experiments instituted by you during 'the last summer have given an explanation of many 'of them.

'Allow me, on this occasion, to acknowledge my 'indebtedness to you, for the instruction which I have 'received; for after the conversations which I had 'with you, and after having witnessed a number of 'your interesting experiments on the blood, I feel perfectly satisfied, that in *fever* the blood and vascular 'system is the chief seat of the disease. I am now 'also satisfied, that in malignant fevers the disease 'can only be cured by the proper use of those medicines that possess the power of acting on the blood, 'and that prevent or remedy its diseased condition.

'That your health may long be preserved, and 'that the world may soon be in possession of your 'thoughts and experiments on these subjects, is 'most sincerely the hope of your friend,

'A. HOSACK.'

'Dr. William Stevens.'

'*New York, Jan. 7th,* 1831.

'My dear Sir,

'While we were residing together at 'Saratoga springs, during the month of August last, 'I recollect perfectly that we had at that time several 'conversations relative to the blood, and the changes 'which that fluid undergoes in the lungs. You then 'mentioned to me that you had long been aware of 'the fact, that the oxygen or something else in the 'atmospheric air, had a strong affinity for carbonic 'acid, and that the oxygen would attract the acid, 'even through the medium of a dense membrane, 'and to this you attributed the purification of the 'blood in the pulmonary organs. You also told me 'at the same time, that the experiments which you 'had made at the High Rock, proved beyond all 'question that this was the case. I regret that my 'other avocations prevented me from being present 'at these experiments; but I have the most distinct 'recollection of the above conversations, and from 'them I was aware of the circumstance a considerable time before any of the recent publications on 'the same subject made their appearance in this 'country.

'With sentiments of unfeigned regard,

'Yours truly,

'H. M'Lean, M.D.'

'William Stevens, M.D.'

'*New York*, 31*st Dec.*, 1830.

'Dear Sir,

'I recollect perfectly well, that, on your 'return to this city from the western district, in the 'month of *October* last, we had several conversations 'on the subject of the changes which the blood 'undergoes in the pulmonary organs. You then 'mentioned to me, that during your residence at 'Saratoga you had made a number of experiments 'with the natural carbonic acid of the High Rock, 'and that those experiments had manifestly proved 'that oxygen had a strong attraction for carbonic 'acid; that though there might not be any new 'chemical combination, yet there could be no question 'of the fact, that the oxygen possessed this attractive 'property to such a degree, that it had, at the High 'Rock, removed the carbonic acid from tumblers, 'and other vessels, even when a dense membrane 'had been interposed between them. From these 'and other facts you inferred that a similar pheno-'menon occurred in the pulmonary organs. A few 'days after, and while you were still in New York, 'I had a conversation on this subject with a gen-'tleman, who is considered one of the best practical 'chemists in America. It was then decidedly his 'opinion, that you had made some error in your 'experiments, as the result was so much at variance 'with the laws of chemistry at present known. I 'may add, that as the above conversations took place 'during the month of October last, they were prior 'to the recent publications in this country on the 'same subject.

'Believe me to be, your's,
'With respect and esteem,
'A. E. HOSACK.'

'To Dr. Stevens.'

The power, whatever it may be, which enables oxygen to attract and lift up carbonic acid, even through the medium of a membrane, is one of intense interest to the physiologist, as well as the practical physician: and afterwards I may take some pains to prove, that I was perfectly aware of the existence of this power as far back as the year 1827.

But, in the mean time, I trust that I have produced evidence enough to convince my readers, that if this be a discovery, at all events I have not borrowed it from the Philadelphia writers; for I had not only stated the above fact to many individuals in Europe, before I went to America, but the experiments at Saratoga were made nearly three hundred miles from Philadelphia, in the month of August, 1830, while their papers were not published until the following November; neither, even by their own statement, were their experiments commenced on this subject, until the 21st of July, that is, immediately after I had mentioned the existence of this power to several of the scientific physicians in that country, and that too, as I have said, before either Dr. Faust or Dr. Mitchell had made a single experiment on the attraction of gases.

VITALITY OF THE BLOOD.

The vitality of the blood is not a new doctrine; it was openly declared in the sacred writings; it was more than hinted at by Hippocrates; Harvey believed in its vitality, and the same opinion was warmly adopted by the late Mr. John Hunter. Since then, this doctrine has slowly but steadily been gaining ground, and the facts which support it are so strong, that this belief may now be considered as almost universal.

It is difficult to define life: for we know not what it is; and this is the secret of all others, that nature will be most likely to keep to herself. We may give it a name, however, and call it what we will; but the very existence of this mysterious essence, like the cause of heat, is known only by its obvious effects. But these effects are evident to all; we know, for example, that this is the cause which constitutes the difference betwixt the living and the dead body, and the loss of which is the cause of death. We know that, for a time, it endows matter with certain properties; such, for example, as the power of self-preservation. It enables all living animals to resist, within a certain range, the effects of noxious agents, as well as those chemical and other changes that would be injurious to life. It enables them not only to provide for, but to regulate their own temperature. But, even independent of the property of evolving heat, it enables a living animal, in a certain degree, to resist the effects of extreme cold. It is this which enables the blood, when it forms the new solids in the living body, to organise itself; it is this, also, which endows the whole of the animal creation with the power of communicating vitality to their offspring, and each in return to continue the existence of their own race. Such are some of the characteristics of life; and the blood is as essential in the production of these phenomena as either the brain, the nerves, or any of the solid structures in the animal economy.

When more agents than one are concerned in the production of certain effects, we ought not to consider any one link in the chain as the sole cause; for all the animal functions act in a circle, and are mutually dependent upon each other. The vascular solids contain and circulate the blood; but the blood is the main spring of life, and the vascular organs are indebted to this mysterious fluid, not only for their nourishment, but also their motion. For these solids are not merely formed from the blood, but this is the active and living stimulus that communicates to the heart and vessels that permanent impression, which is, in reality, the true cause of their incessant action.

My gifted friend, the late Mr. John Bell, who argued with so much warmth against the vitality of the blood, allowed, however, that water was the life of the mill; for when this is withdrawn, the machinery almost immediately ceases to move. On the same principle he ought to have admitted the vitality of the blood; for when the vascular organs are supplied with their natural stimulus, the heart continues to act; and this, as well as the other solids, retain their vitality, even for many hours after the head of an animal has been cut off. But the brain cannot perform its functions even one minute without blood, and when this is withdrawn, either from the brain or any of the organs, their vitality is immediately lost, and the whole of the solids instantly die, 'for in the blood is the life thereof,' and without this, even the brain and the nerves become instantly mere inanimate matter.

The vitality which exists in the solids, independent of the brain, has been called lately the nervous principle; yet it may be proved, that this is not derived from the nervous system, for the diffused principle of life, like the nerves themselves, and everything else in the living body, is derived from the circulating current. It is said to be confined to particular organs; but this, I believe, is also an error, for the vital principle, like the blood, is diffused, but not equally, all over the living body.

It is the blood that forms and first stimulates the heart into action; this, therefore, may be justly considered as the *primum mobile*. The voluntary muscles derive their vitality from the blood, but that electric fluid which stimulates *them* into action, is evidently the effect of the nervous system. The nerves, however, are not essential, either to the vitality of a part, or to the formation of the solids. Fetuses have been born without either a brain or spinal marrow, yet, in every other respect, they were fully formed; and in those limbs that are completely paralytic, the blood is converted from arterial to venous and evolves heat, nearly as fast as in those parts of the body where the nervous communication is not interrupted. The limb loses its sensibility, for the nerves now are not capable either of receiving or communicating impressions to the brain, but still it continues to live. In the process of time the muscles become less plump—this, however, is the effect of the want of action; but the motionless limb, so long as it is nourished by the blood, continues to live, until life ceases in the whole system.

'It is difficult,' says Mr. Hunter, 'for minds not 'accustomed to reason on this subject, to conceive 'the idea of fluid vitality;' but this difficulty is no proof against the doctrine of the vitality of the blood: for it is equally difficult for a native of the tropics, who has never seen ice, to conceive the possibility of water becoming solid; or for an Indian, who has never seen glass, to believe in the possibility of a solid substance being perfectly transparent. But still, notwithstanding this difficulty of conception, water becomes solid at 32°, and objects, in some cases, can be seen much better through the medium of a glass than with the naked eye.

The vital principle is diffused, but, as I have said, like the blood, it is not equally distributed through every part of the living system; and those physiologists take but a mean and contracted view of the animal economy, who suppose that vitality can only reside in a solid structure. But even the blood is only a fluid, so long as it circulates in its own vessels, for the vital current contains ingredients that are naturally of a solid consistence; and, may we ask, what are the solids, even the nerves and the brain, but a part of the living blood which has left its own vessels, assumed the solid form, and organised itself, in one part in one way, and in others differently, so as to form brain, nerves, bones, muscles, membranes, and the various solid structures of the body; but all of these are derived from the blood, and without this they cannot live.

The principle of life, whatever it may be, is not a solid, neither is it confined to the solid structures: like the cause of heat, or electricity, it exists in the shape of an invisible imponderable fluid. We know that caloric exists in the vital current; and I can see no reason to doubt, why the living principle may not, like heat, exist in the blood, to as great a degree as in any other part of the animal frame.

Life is, perhaps, more essentially necessary in the blood than it is in the solids, and, for my own part, I can no more imagine it possible for caloric to exist merely in the solids, and not in the blood, than believe that the vital principle exists only in the one and not in the other; it is true that the grosser ingredients in the circulating current are derived from inanimate matter, but this is no proof against the doctrine of fluid vitality; and the same argument might be used with equal propriety against the life of the solids; for is it not from these grosser ingredients that the solids themselves derive their existence? The vital principle assimilates, for a time, the grosser ingredients to itself, and for a certain period endows them with life. It is not, however, this inanimate matter which gives life to the blood, but the vital principle of the blood that gives life to the grosser ingredients, not only in the blood itself, but in the whole system.

It is now well known, from the important discoveries of Sir Charles Bell, Magendie, and Mayo, that there is one class of nerves which convey impressions to the mind, not only from external objects, but from every part of the living frame; and another distinct class which convey impressions (probably electric) from the mind to the various parts of the system. That a living animal can possess an apparatus within itself, capable of producing effects similar to those of the electric fluid, no one can doubt, who has felt the shock of an electric eel; but whatever the real nature of that agent may be, which enables the nervous chords to communicate or convey impressions with so much rapidity, still this agent itself ought not to be identified with the living principle, which exists in the blood, in the brain, and all over the system; for life is the cause and the nervous fluid is merely one of the many mysterious effects which are produced by the principle of life, which is, as I believe, not a material agent. This, for a time, may assimilate animal matter to itself, and when the habitation that contains it is assailed by noxious agents, or deranged by disease, the living principle may change its residence, but it cannot perish.

Mr. John Bell denies the vitality of the blood, yet it would not be difficult to prove, even from his own admissions, that the blood possesses life, for he admits that it becomes vital by exposure to the air in the lungs. He states also, that the blood loses its vitality during coagulation; for, he says, that after it coagulates the blood is dead. Now it is very clear that, if the blood had not previously been endowed with vitality, it could not die on being removed from the system.

It was ascertained by Mr. Hunter that, when we take two bodies, *the material ingredients of which* *are exactly the same*, if the one be endowed with vitality, and if the other be dead, the former possesses the power of resisting heat, cold, and chemical changes, while the other does not possess this power to a greater degree than any other inanimate substance; such is the case when an animal dies; consequently the mere composition of matter does not give life, for a body that is recently dead has all the material composition that it ever possessed.

When we take two incubated eggs of the same size and of the same age, and expose one of them to intense cold, so as to destroy its vitality, and then apply an equal degree of heat to them both,—the one that has lost its living principle will undergo chemical changes and soon become putrid, while the other which still possesses its vitality will not only resist the effects of a temperature of all others the most favourable to putrefaction, but, in a short period, a living animal will start into existence.

If the power of evolving heat be a proof of vitality, we may then conclude that the blood is alive; for it not only evolves its own caloric, but it is the chief agent in supplying the solids with that heat which is the true cause of their high temperature. It may be said, however, that the carbon is derived from the solid structures; but, then, this inflammable matter is itself, like everything else in the solids, originally derived from the vital current.

It has been proved by Mr. John Hunter, that the blood, when just drawn from a living body, possesses, within a certain range, the power of resisting cold; for blood that had been previously frozen and then heated to the same degree, cooled much faster than blood which had just been drawn from a living individual. I believe there is little doubt of the accuracy of the above experiments; and, if so, there can be little doubt that the circulating current possesses life, even for a few minutes after it is drawn from the system.

It has been supposed that the blood derives its vitality from the oxygen of the air; there must, however, be some other source. It is true that oxygen is essential to the continuance of life; for without this there can be no evolution of animal heat, and the blood in the lungs would not be purified; but oxygen can no more give origin to vitality, than it can, of itself, produce heat. When carbon and oxygen are brought into contact in a low temperature, there is neither combustion, heat, nor flame; but when an agent, already in a state of ignition, is applied so as to raise the temperature of the carbon to a certain point, it then combines chemically with the oxygen, heat is evolved, and the combustion continues, at least so long as the materials last. Such is the case in common combustion, and such is the fact with respect to the evolution of animal heat, a process which is essential to life, particularly in all animals that have warm blood. But, though oxygen and heat be essential to, they are not identical with, life; for those creatures that have the coldest blood and consume least oxygen, are endowed with animal vitality to a greater degree than those animals that consume most oxygen during life; and, when they die, heat, and that very oxygen which had been so essential to the continuance of life, are now, 'when the spirit is fled,' the very agents of all others the most active in producing those chemical changes which are the cause of dissolution; for when these are excluded, putrefaction may be prevented, even after the loss of the living principle, or, if it does occur, it goes on but very slowly.

It has been asserted that vitality prevents the occurrence of chemical changes; we have seen, however, that there are chemical changes incessantly going on in the living system, and those very changes are essential to life. It is only to those chemical changes or noxious agents that are injurious to the system that the vital principle opposes its force; and the stronger an individual is, or, in other words, the more he is endowed with vitality, the more he possesses this power of resisting the effects of noxious agents, or those chemical changes that are destructive to life.

When two individuals, for example, take, at the same time, a large draught of cold water from the same pump, if the one be weak and exhausted, the stomach is suddenly deprived of its natural heat, the vessels lose their power of action, the blood becomes suddenly cold and ceases to circulate, the stomach is instantly chilled, and the impression is immediately communicated to the heart, to the brain, and the whole system; the circulation stops, and the individual drops down dead. While in the other, the impression produced on the stomach by the cold water is immediately followed by re-action in the extreme vessels; an extra portion of caloric is evolved, the cold fluid in the stomach is soon heated to the temperature of the body, and the individual, so far from being injured, leaves the spot refreshed by the draught.

We know also that when two individuals go into an infected district, where the whole atmosphere is completely saturated with an aërial poison, if the one be weak and the other strong, the one who has least vitality will be almost sure to be attacked, perhaps in a few days, with fever; while in the other, though an equal quantity of the poison may enter the system, yet this is probably thrown out of the circulation through the medium of the secreting organs, and in this case he may escape altogether, or if he be attacked, the poison takes a longer period to produce its effects. In the person who is weak the re-action is less, but the symptoms in general are more malignant; while in the other, there is greater re-action, but less danger, for the blood is not so much injured by the noxious agent.

I have formerly stated, that whilst the blood is circulating in the vessels, both the fibrin and the albumen owe their fluidity to the circumstance of their being held in solution by a saline fluid; and so long as the vital current is in active circulation, its vitality prevents the occurrence of those changes which are the cause of its coagulation when removed from the system. But when a part of the blood is drawn from the body, it soon dies, and becomes solid, as, I believe, almost exactly in proportion as it loses its living principle.

When blood has been completely deprived of its vitality, either by poisons, extreme cold, the electric fluid, or from any other cause, the fibrin of even this dead blood will become solid to a certain extent, particularly when we expose it to the air; but the half solid soft coagulum which forms in such cases, is very different from the fibrous, dense, and firm crassamentum which is formed, when healthy blood has just been drawn from a healthy individual. It is also well known, that when a person is weak, the chemical changes commence much sooner than in healthy blood, where they are, at least, retarded for a time by the vital principle; but still, in the end, the crassamentum is much firmer in the blood of a healthy individual than in those that are weak.

Mr. Hunter acknowledged that even dead blood becomes solid; and, having done this, we are at a loss to conceive how he could consider coagulation as the effect of the vital principle. Common coagulation is the effect of changes, which are going on whilst the blood is losing its vitality; consequently, it is not the living principle but the diminution, or loss of vitality, which is the cause of the change.

Still there are circumstances connected with this process, which afford a strong proof that the blood, in its healthy and natural state, is endowed with life, even for a short period after it is drawn from the living body.

The blood of health is not dead at the moment that it is drawn, and it only begins to become solid, when its vitality is so far diminished as to render it incapable of resisting the effects of certain changes which occur when the removed blood is allowed to rest; but still even when coagulation begins, the fibrin of healthy blood retains so much of its vitality, as to enable it to contract and form a firm fibrous crassamentum, which is very different from the soft gelatinous consistence of the coagulum, which forms when dead blood is taken out of the heart and exposed to the air.

When there is inflammatory action going on in any part of the system, the fibrin of the drawn blood contracts with such firmness, that it frequently produces a cupped appearance on the upper surface; and, even in health, the blood of the strong contracts much more firmly than the blood of those who are weak: in fact, the firmness of the coagulum depends almost entirely on the degree of vitality which the blood possesses at the moment when it is drawn from the body.

The muscular fibres are formed from the fibrin, and so long as these retain their vitality, they contract firmly on the application of salt, or any other stimulus; but when they lose their vitality, they are not affected by the application of stimuli. When we mix salt with the blood, before the fibrin begins to coagulate, it is prevented from becoming solid, by being dissolved in a fluid that is so much richer in saline matter than common serum; when we allow the fibrin, however, to assume its solid form, it acquires new properties, and is no longer soluble, even in a saline fluid. But if, at a certain period after the coagulation has commenced, we add muriate of soda, or a saline solution, to the coagulating blood, the moment that the fibrin feels the stimulus of the salt, the whole of it becomes suddenly solid; and I have seen the fibrin of inflammatory blood, which had been drawn during the hot stage of the marsh fever, contract, on the application of salt, with almost as much rapidity as the muscles, when we apply the same stimulus to the fibres in the living body. This, of itself, is a strong proof of the vitality of the blood; for the fibrin of dead blood will no more contract on the application of stimuli than the muscular fibres of an animal that has been dead for many days. Now, if the blood, even out of the body, possesses the power of resisting, to a certain extent, the effects of heat, cold, noxious agents, and chemical changes, that would be injurious to life—and if the fibrin of the living blood possess sensibility to such a degree as to feel and re-act on the application of stimuli,—we may then infer, that the blood possesses life within itself, which exists totally independent of either the brain or the nervous system; for there can be no nerves in the vital current, and blood that is drawn from the body cannot contract or show signs of life from sympathy or any connexion with the cerebral organs.

When a warm-blooded animal is bled to death, the heart almost immediately ceases to contract, from the want of its natural stimulus to keep up its action; but when we cut off the head of another animal of the same class, secure the vessels in the neck, and keep up an artificial respiration, so as to purify the blood, the heart retains its vitality, and continues to act with force, even for many hours. The blood continues to circulate, and the solids not only retain their vitality, but the muscles continue to act with great power, even for a long period after the head of the animal has been fairly separated from the body. That such is the fact is evident from what occurred in the celebrated experiments of Mr. Brodie. We have seen, also, that when the structure of the brain is completely destroyed, and the chest of a rabbit is opened immediately, so as to expose the blood in the large vessels, &c., to the action of the air, the auricle continues to live, to feel, and to re-act on the blood, for three hours, even after the brain has been converted to a perfect pulp. Now, from this alone we may infer, that the solids are not indebted to the brain for their living principle; for if the nerves, either of themselves, or from their connexion with the brain, be the cause of animal vitality, why is it that the solids instantly die when deprived of their blood; and that, too, even when the brain and the nerves are left uninjured? On the other hand, that the solids derive their life from the blood is evident from the fact, that the heart, and the other vascular organs, as well as the muscles, &c. &c., continue to act so long as they are supplied with pure blood, and retain their vitality for many hours after the brain of the animal has been reduced to a perfect pulp, or even after the head is removed from the body;—consequently we may infer, that the blood in its healthy state possesses life, and that the solids themselves can only live and retain their vitality so long as they are supplied with their living fluid, and when this is withdrawn, death must immediately follow in the heart, the brain, in the nerves, and the whole body, 'for the life of the flesh is in the blood,' and without this they instantly become mere inanimate matter.'

ON THE MODUS OPERANDI OF AGENTS ON THE LIVING BODY.

It has been asserted, that 'the process of assimilation animalizes all articles subjected to its influence*.' We know, however, that this is not the case, for many agents enter the circulation unchanged, mix with the blood, and some of them are again thrown out of the circulation in their original state. All the natural salts of the blood enter the circulating current, and may be obtained in their original form, either from the serum or the secretions; but even others, which are not natural to the blood, enter the circulation, and after a time they are thrown out of the system unchanged in any of their properties. Nitre, for example, passes through the digestive organs, and enters the circulation without being converted into an animal substance. Alcohol is not animalized by the nerves, for it has been found so pure in the ventricles of the brain, as to be even inflammable when exposed to heat. Mercury makes the bowels irritable, though it be only rubbed into the skin; and even when taken by the mouth, part of it passes out of the circulation, dissolved in the fluid of perspiration, and forms an amalgam with the gold or silver that may chance to be in the pockets of those who are impregnated with this active remedy. When sulphur is taken into the stomach, it enters the circulating current, mixes with the blood, and, after a time, it is thrown out by the skin, so little changed in its properties, that it blackens silver in the pockets of those who have taken this remedy, even by the mouth. Now, it is very evident, that these agents are not animalized, neither is it sympathy that produces the amalgam; for sympathy cannot blacken silver out of the body, or give a white colour to a piece of gold.

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* *See* Chapman's 'Therapeutics.'

Notwithstanding the innumerable proofs to the contrary, it has been maintained, even very lately, ‘that all poisons, and, indeed, perhaps, all agents, ‘influence the brain and general system, through ‘an impression made upon the sensient extremities ‘of the nerves*.’ Now those who maintain that all poisons, and, perhaps, all agents, influence the brain and the rest of the body, only by a direct impression on the nervous system, might tell us at the same time with equal propriety, that oxygen in the lungs purifies the blood through the medium of the nerves, and not by removing the carbonic acid from the venous current; or, that the food which we use nourishes the system, not through the medium of the thoracic duct, but by some mysterious impression made first on the nerves of the stomach, which is then conveyed to the brain, and afterwards by sympathy to the rest of the body. That certain agents produce their effects only on the nerves is extremely probable; but, that many deleterious substances produce, and that many remedies cure disease, through the medium of the blood, is, I believe, just as certain, as that oxygen attracts carbonic acid through the medium of the pulmonary membrane, or that the food which we use is converted into chyle, and nourishes the solids, not by any mysterious impression on the nervous system, but entirely through the medium of the vital current.

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* See Morgan and Addison on Poisons, p. 90.

There are certain noxious agents that are almost inert, so long as they are merely applied to the sensient extremities of the nerves. The poison of the rattle-snake, for instance, is perfectly harmless when applied to the eye, the tongue, &c.: it may even be taken into the stomach with the most perfect impunity. I have given it internally, in large doses, to foxes, monkeys, dogs, and other animals; but still it produced no apparent effect. Yet, when a small quantity of the same fluid is inserted into a recent wound, or injected into a vein, it causes death in a few minutes.

The rattlesnake has two fangs, which are concealed in a sheath, one on each side of the upper jaw; they are curved in their shape, and their point is as sharp as that of a common needle. They are hollow in the centre, and the root of the fangs are connected with the poison-bags. These reptiles generally use only one fang at a time, and when they do use it, they seize the part with their mouth which they intend to poison, they then perforate it deeply with the fang. At this moment the bag contracts, and the deleterious fluid which has such an enmity to the blood is injected into the very bottom of the wound, through a small aperture in the under part of the fang, at a short distance from the sharp point. Having effected his purpose, he withdraws the instrument, and then leaves his victim to his fate.

When a small animal, a rabbit, for example, is poisoned by this snake, it does not seem to feel pain at the moment; and, generally, for the first five minutes, he appears to be perfectly well. At the end of this period, however, the ears begin to droop, he seems giddy and uneasy, the lower extremities soon lose their power, he falls on the ground: the pupils dilate, slight convulsions come on, and the animal dies, generally in about fifteen minutes from the time that the poison had been injected into the wound.

When we examine the part immediately after death, we find that the poison has completely destroyed the red colour of the blood, and not only of this, but for two inches all round the puncture, the muscular fibres, and even the cellular substance are as black as if they had been for hours in a state of complete mortification. The blood in the veins which lead from the wound is also black, and even that which is taken from the heart is darker in colour, and does not coagulate so firmly as in those animals that are killed by a blow on the head. When we examine the head, after death, there is a want of blood in the brain, similar to that which occurs in those persons who die during the cold stage of fever; but neither the brain nor any of the solids appear to have sustained the slightest injury, except at the wound, and this, of itself, is not sufficient to account for the suddenness of the death. But the blood is diseased even in appearance; and my belief is, that this poison, like most of the others, produces death, not by any direct impression on the brain, but by suddenly depriving the blood of its vital principle; for the solids cannot continue to live when that dies which is, in truth, the cause of their vitality.

The exact time that this poison requires to produce its effect depends, in a great measure, on the quantity of blood contained in the part to which it is applied. When injected into fat or other substances, where there is little blood circulating in their texture, the poison appears to be almost inert. It is, probably, for this reason, that hogs attack even the most venomous snakes with perfect impunity, and eat them, poison and all, without suffering in the least. On the other hand, when a full-grown rabbit is bitten in a fleshy part by a large rattlesnake, death generally follows in from twelve to fifteen minutes; but, in one instance, where the animal died in three minutes and a half, on examining the part, I found that the fang had pierced a vein of considerable size.

Fatal as this poison is when it mixes with the blood yet when we cut out the black flesh from the poisoned wound, and give it to a dog, or any other carnivorous animal, he eats it readily, and takes it into his stomach with the most perfect impunity. Or, if we saturate a piece of bread with the poison, he eats it, without suffering in the slightest degree. Nor is it any chemical mixture betwixt the blood and the poison which causes death; for I have mixed the fluid with recent blood, and dogs ate it readily without being poisoned. It is, therefore, only fatal, when it mixes with the living blood, in a living animal, and causes death, as I believe, by destroying the vitality, first of the blood, and then of the whole body; for this fluid does not act as a poison when applied to the sentient extremities of the nerves, and the mere organic injury which is done to the solids is not of itself sufficient to account for the sudden death.

There are more noxious agents than one that may be perfectly innocent in one part of the system, but deadly in their effects when applied to another. A certain quantity of carbonic acid, for example, may be taken into the stomach with the most perfect impunity, yet the same agent causes almost instant death when taken into the lungs*. Blood containing carbonic acid produces no evil effects on the internal surface of the right side of the heart, yet the same acid acts as a poison when the blood that contains it passes into the left ventricle, or the coronary arteries, without being purified in the pulmonary organs. The juice of the Wouralli vine, like the venom of the rattlesnake, is not a poison when merely applied to a nervous surface, yet it causes almost instant death when it mixes directly with the living blood. We all know that the poison of the viper can be swallowed with impunity; we have seen also that the same is the fact with respect to the venom of the rattlesnake; for these agents act only as *poisons* when they mix with the *blood*: yet though they are not poisonous even in large doses, still it is probable that they are not altogether inert when they are taken into the stomach; for there, they are probably changed in their properties by the gastric juice before they enter the circulation, either by the veins, or as part of the chyle. Mr. Wallace, of Virginia, took the whole of the poison from the two fangs of a large and vigorous rattlesnake. This he made into pills, 'bags, venom, and all.' These he swallowed himself, sometimes at the rate of four a day. They produced, he says, most heavenly sensations, and 'melancholy was quickly changed into gay anticipations*;' but un-

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* It is possible that carbonic acid in its most concentrated form may not enter the chest; but when it is so strong as to kill a rabbit in three minutes and a half, it enters freely into the lungs, and, probably, produces a complete spasm in the cells. But, independently of this effect, as carbonic acid cannot remove the cause of the impurity from the blood in the pulmonary

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organs, this of itself is quite enough to account for the sudden death which ensues when an animal is immersed in an atmosphere of this gas.

* See Coxe's American Dispensatory, p. 690.

fortunately, these delightful feelings were followed by a general dropsy, which continued for a considerable period. However, he is still very sanguine about the virtues of this venom, when used internally, and thinks the time is not very distant when rattlesnakes will be reared for medicinal purposes, as much so as 'either the poppy or palmachristi.'

I may here observe, that when the muriate of soda is immediately applied to the wound that is made by the rattlesnake, it is a complete antidote against this poison. When an Indian is bitten by a snake, he applies a ligature above the part, and scarifies the wound to the very bottom; he then stuffs it with common salt, and after this it soon heals, without producing any effect on the general system.

The poison of the rattlesnake produces an immediate and obvious effect on the blood, as well as on the solids of the part to which it is applied; but there are other noxious agents which cause death through the medium of the circulating current, without producing any apparent change, either where they are applied, or in any one part of the solid structure. Such is the case with the Wouralli poison: for this noxious agent, like the venom of the rattlesnake, produces no deleterious effect when merely applied to the sensient extremities of the nerves, even on those surfaces that are most sensible. I have given it internally, in large doses, to dogs, monkeys, and other inferior animals, yet they did not suffer, even in the least; but when we dip a small wooden needle into this poisonous juice, and then puncture with this either the flesh or any other part of the body that contains blood, almost the very moment that the poison mixes with the living blood, it destroys its vitality in the part to which it is applied; and, if the communication with the rest of the current be not immediately prevented, this loss of vitality in one part of the blood, almost instantly extends to the whole, and the animal dies, generally in less than fifteen minutes.

When we examine the spot that had been punctured with the poisoned needle, there is *less* appearance of inflammation or disease than if the part had merely been pricked with a common pin; and when we examine the rest of the body, there is not the slightest trace of disease to be seen, in any part of the solid structure. Now, in this instance, it is very clear that death is not produced from any increased action, or organic disease, in the vascular organs; and, as we know that this poison *does not possess the power of producing any direct impression on the nervous system*, we may infer that it causes sudden death by the immediate destruction of the living principle of the blood, in the part to which it is applied, and this loss of vitality in the circulating current extends almost instantly to the whole body.

It is life alone, which they derive from the blood, that enables the solids to feel and re-act on the application of the nervous fluid, or any other stimulus; and when this is lost, they are no longer capable of receiving impressions; but, independent of this, when the blood dies, it is no longer a stimulus to its own vascular organs. It is not, therefore, necessary that the poison itself should be carried to the brain; for if the noxious agent destroys the vitality of the blood, or stops the action of the heart, it will put an end to the functions of the brain, as suddenly as if the poison were injected directly into the carotid artery: neither is it necessary that the impression of the poison should be conveyed by the nerves to the cerebral organs; for if the blood loses its vitality, it is very evident that this dead blood can no more stimulate the heart and vascular organs, than these organs can perform their healthy functions without a regular supply of healthy blood; and when the poison has destroyed the life of the blood, the vascular solids cease to act, not from any injury done to their solid structure, but from the loss of vitality in their stimulating fluid, and without which the heart, the brain, &c., are altogether incapable of keeping up their action, or supporting life.

I have formerly stated, that when we destroy the structure of the brain of a rabbit, and immediately open the chest, so as to expose the lungs, the great vessels, the auricles, &c., to the action of the air, the heart continues to act for, at least, three hours after the brain has entirely ceased to perform its functions. But if, during this period, we inject a few drops of prussic acid, or any other fluid poison into the jugular vein; this poisoned or dead blood, even under such circumstances, produces an obvious effect on the action of the heart, for it almost instantly destroys its vitality, and puts an end to its action. Now, this is evidently not the effect of a *morbid impression*, conveyed first to the brain by the nerves, and then to the heart by means of sympathy, for the structure of the brain had been reduced to a pulp, previously to the injection of the poison into the blood.

In females who have died during pregnancy, from the effects of some deleterious poison, the same specific agent which the mother had used, has been found in the blood which was taken from the heart of their dead infants. It is also the fact, that in small-pox, measles, the marsh fever, &c. &c., the same specific disease is frequently communicated, through the umbilical circulation, from the mother to the child, before birth; and after this, the disease in the child may be effectually cured by remedies which are used by the mother, and pass through the maternal circulation into the glands of the breast, and from these into the vascular system of their infants. These remedies not only cure disease in the child, but the very agents themselves have been detected in their blood. Now, this cannot be the effect of a nervous impression, for though the nerves may convey the impression which is made by an agent from one part of the body to another, yet they have no tubes, and, of course, cannot convey *the specific agent itself* from the stomach of the mother to the blood of her infant, either before or after birth.

That the solids are frequently the sole cause of disease is, I believe, beyond all question; but it is equally certain, that a vitiated or diseased state of the fluids is frequently the sole cause of disease in the solids. When a fluid poison, for example, is taken into the stomach, is it not the fluid, in this case, that is the cause of the diseased action in the solid structure? When the acrid principle of cantharides is absorbed from the skin, and thrown out by the kidneys, is it not the acrid quality of the urine that produces the diseased action in the bladder? Or, when we inject a small quantity of putrid water into a vein, is it not this putrid fluid that deranges the blood, and produces fever of a more malignant type than those that are generally met with in cold countries?

When an individual receives a severe blow on the head, so as completely to destroy the functions of the brain, respiration is immediately suspended, but the heart continues to act feebly for a time, and a part of the blood passes on through the lungs for a short period, without having been purified in the pulmonary organs. In this case it is the injury done to the solids which is the cause of death, and the dark colour of the blood which is found in the left side of the heart. On the other hand, when a person breathes in an atmosphere of nitrogen, the carbonic acid in the circulation is not removed, but the blood, for a time, passes on to the heart, without being purified in the lungs. In this instance, it is evidently the diseased state of the blood which is the cause of death, for nitrogen has no noxious property; but it has no attraction for carbonic acid, and neither the heart nor the brain can perform their functions when supplied with blood that contains an excess of this gas. The left side of the heart is the first part of the solids which feels the effect of this impure state of the vital fluid, for the blood, under such circumstances, may still circulate in the lungs for a short period, but so slowly, that the quantity which arrives in the left ventricle is considerably diminished, and that which does arrive is not pure; consequently, though the heart continues to contract feebly for a time, it sends but a small quantity of blood into the aorta; and that which passes through the cerebral arteries is not only small in quantity, but so impure in quality, that the brain almost instantly ceases to perform its functions, not from any injury done to its solid structure, but merely from the diminished quantity and the impure quality of its nutritive fluid. The pupils dilate, the individual becomes slightly convulsed, and soon dies with symptoms similar to those which occur when an animal is bled to death *.

When a person breathes in an infected atmos-

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* Persons who were apparently recovering from drowning have fallen down and died suddenly. This is, probably, the effect of the impure blood on the brain.

phere, a part of the aërial poison may be attracted with the atmospheric air into the arterial blood, or even admitting that it mixes with the saliva, and enters with the chyle, still in either case it enters the circulation. In this instance, as well as in many others, it is probable that the diseased state of the blood is the sole cause of the diseased action in the vascular solids; for it is this black and poisoned blood which, after a time, paralyses the heart, and, in fact, the whole of the vascular organs. It is this that causes the cold stage, and diminishes, for a time, the functions of the brain, as well as of every other organ in the system.

I shall afterwards endeavour to prove that all the essential or idiopathic fevers are primarily produced by a diseased state of the whole circulating current. But, independent of these, there are many other diseases, where the blood is infinitely more concerned, as the immediate cause, than any of the solids; and, in such cases, the solids suffer merely as an effect of the diseased state of their nutritive fluid.

The embryo which contains the rudiments of the future being is a small speck, which is formed by a secretion from the maternal blood; while the fluid which stimulates this into life is also a secretion from that of the opposite sex. When the parents are both healthy, their children are free from hereditary disease; but when either of the parents is tainted, this taint is as effectually communicated to their offspring, as either the cast of the countenance or the colour of the skin. The disposition to gout, scrofula, leprosy, the venereal, and other hereditary diseases, can only be communicated through the medium of the fluids. In this way, 'God visits the iniquity 'of the fathers on their children, down to the third 'and fourth generation.' In the above diseases, the vitiated state of the blood is, probably, the sole cause of the morbid action in the solids; and when we attempt to cure them, we may not, as yet, always succeed; but we best gain our end by the use of those medicines that act not on the nerves, or, in a part, but first on the blood, and then on every solid in the system, through the medium of the circulating current.

When the venereal poison is applied to the skin, it causes at first a local disease, but, after a time, it is observed to travel to other parts of the system, producing the identical same disease in the throat, the nose, the skin, and even in the bones of the lower extremities. Now are we to believe, without proof, that the specific poison is conveyed from the original sore to the throat, the nose, or the leg, by means of sympathy; or are we to infer, from the slowness of its action, the inflamed state of the lymphatics, and other symptoms, that the poison itself is carried either by the absorbents or the veins into the general circulation, and then by the arterial blood to the various textures upon which it fixes? And, when we know that the antidote to this poison also enters the circulation, so as to come in immediate contact with the poison itself, are we to believe, under such circumstances, that it destroys the cause of the disease by sympathy, or are we to infer that it acts upon the poison by actual contact, and renders it inert through the medium of the blood, in which the antidote is diffused as well as the poison which causes the disease?

The fact that mercury does enter the circulating current, rests on proofs too solid to be shaken by mere assertion. Dr. Hamilton found mercurial globules in the milk of a woman who was then in a state of salivation. Zeller found them in the bile; Wepfer, Laborde, &c., have detected them in the bones, while others have found distinct globules in the substance of the brain, the mammary and the salivary glands, even where the mercury had been rubbed upon the skin in the form of ointment.

It has been proved by the important experiments of Tiedeman and Gmelin, that the lacteals absorb, in preference, all the common nutritious matters, while many of the most active agents enter directly into the circulation without passing through the thoracic duct. These are either absorbed from the stomach and intestines by the roots of the vena portæ, and carried directly into the circulation, or they are attracted, through the thin coats of the veins, into the current, and mix directly with the circulating blood. *When an excess of saline matter is taken into the stomach, the greater portion of it passes directly into the right side of the heart, and from that almost immediately to the whole system.* Alcohol passes so rapidly into the circulation, that when an animal is forced to drink diluted spirits during digestion, the spirit immediately mixes with the blood of the vena portæ, and gives it a strong odour, while not one particle of the alcohol is left to mix with the chyle. When a diluted spirit is injected into the vein of an animal, it produces intoxication in a few minutes. It is also a fact that, after death, in those individuals who had died during a debauch, gin has been found in the ventricles of the brain, in so concentrated a form, that it was not merely perceptible to the organs of smell in those who examined the dead bodies, but the spirit produced flame on the application of a lighted taper. It has also been proved that the neutral salts, *when given in small doses*, musk, camphor, assafœtida, gamboge, garlic, madder, rhubarb, &c. &c., pass in part directly into the circulation, through the vena portæ, and partly with the chyle, through the medium of the thoracic duct.

The rapidity with which some substances pass through the circulation into the distant secretions, affords us a certain proof that these agents enter almost immediately into the blood. Stehberger found that madder, indigo, and rhubarb, could be detected in the secretion of the kidney, in fifteen minutes from the time that they were swallowed; gallic acid, in twenty-five, the colouring matter of cherries, and the astringent principle of uva ursi, in forty-five. Turpentine, when rubbed on the skin, appeared in the urine in twenty-five minutes; but when inspired, at the same time, it was found in the secretion of the kidney in fifteen. It is also a fact, that the prussiate of potass enters the circulation, and, in a very short period, gives a blue colour to the valves of the heart. When even water, in excess, is taken into the system, or more than is necessary for the process of digestion, the excess is immediately taken up by the roots of the vena portæ, and thrown out of the circulation, in cold countries by the kidneys, and in hot climates by the skin.

Atmospheric air, the aërial poisons, and all those agents that are attracted into the circulation by the blood in the lungs, must mix immediately with the current, and change the physical properties of the blood according to their character; while those that are attracted into the veins, or are absorbed either by the lacteal or lymphatic vessels, must mix directly with the circulating current. When mercurial ointment is well rubbed into the skin, it is absorbed by the lymphatics, and evidently produces its effects through the medium of the circulating current, as is evident, not only from the length of time that it requires to act, but also from the apparent change which it produces in the blood, as well as in the gums, &c. &c.

When corrosive sublimate is absorbed from a sore, it enters the circulation, deranges the blood, and affects the solids sooner than when the sublimate is taken internally. When arsenic, in the same manner, is absorbed into the circulation, it reddens the colour of the muscular fibres, and produces inflammation in the stomach as soon, if not sooner, than if it had been taken directly into that organ. When Sir Everard Home injected an infusion of colchicum into the vein of a dog, it produced inflammation in the intestines sooner than if it had been given by the mouth. When a solution of tartarized antimony is taken internally, from the time that elapses before it commences to act, it is probable that it produces no effect whatever on the stomach, until it first enters the circulation, and then returns to the gastric organs, through the medium of the circulating current. This is the more probable, from the fact that even when the nerves of the stomach are completely divided, and then when tartar emetic is injected into a vein, it produces irritation in the stomach, and vomiting much quicker than when taken directly into that organ. Gamboge and scammony, when injected into a vein, are thrown out by the intestines, and produce their purgative effects, as completely as if they had been taken directly into the gastric organs. In all these examples, it is probable that these, as well as many other agents, are perfectly inert until they first mix with, and give new properties to the blood. They are then thrown out of the circulation into the various cavities, the emetics into the stomach, the purgatives into the intestines, the diuretics by the kidneys, &c. &c., while the increased action and inflammation which some of them produce in the stomach, the intestines, &c., is merely the effect of the diseased secretions produced by the deranged state of the circulating current.

When active agents enter the circulation, it is probable that, for a time at least, they are equally diffused through the whole fluid; yet different agents act very differently on the various solids. Opium, for example, in a large dose, blackens even the arterial blood, and almosts instantly deranges the functions of the brain. Mercury enters the circulation, poisons the blood, and produces fever in the whole system, but acts with most force on the salivary glands; even a 'solution of gum arabic, infused 'into the blood, will not only stimulate the heart, but 'the stomach, intestinal canal, and other organs, with 'which the heart readily sympathizes [*]'; while turpentine, cantharides, &c., produce little effect on the other solids, but act with great force, and almost entirely on the urinary organs.

Some agents are decomposed in the gastric organs, whilst others pass through the circulation unchanged. Those alkaline salts that are formed by the vegetable acids, turpentine, asparagus, &c., undergo new changes in the system, and acquire new properties before they are thrown out. When the carbonates are used internally (except when they meet with an acid in the stomach), they destroy the natural acidity of the urine, and give it alkaline properties, almost immediately, and as effectually as they do when they are mixed directly, out of the body, with that fluid; but, the whole of the salts that are formed by an alkaline base and the stronger acids, enter the circulation unchanged.

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[*] This, however, is not sympathy; for the same deranged state of the blood which causes the increased action in the heart produces the derangement also in the gastric organs.

There can be no doubt that the poisons of lead enter the circulation, and produce their deleterious effects on the intestinal canal, the muscles of the fore-arm, but particularly on those of the hand. After this, there is not only an absorption of the muscular fibres, but in the few that are left, the red colour is lost, and the fibres become as white as if they had been steeped in a solution of Goulard's extract. I have known these effects produced, even when the acetate of lead had been absorbed from the urethra, and the poison was just as destructive when taken into the circulation in this way, as if it had been taken directly into the stomach. When the nitrate of silver is used internally, it is sometimes, even after a long period, thrown out on the skin, giving to the surface, where it has been deposited, an indelible black colour. When nitre is used internally, it reddens the whole circulating current almost immediately; and, as is well known, match-paper may be made by dipping paper into the urine of a person soon after he has taken a full dose of this salt*. The chlorate of potass gives a beautiful arterial colour to the venous blood, and reddens the gums much faster than mercury. Cubebs enters the circulation, and though it may stimulate the stomach, yet it lessens the stimulating power of the blood, and produces a narcotic effect on the vascular organs; it lowers the action of the heart, and, in some cases, I have seen it cause drowsiness to such a degree, that it was scarcely possible to keep the persons awake. It passes through the circulation with great rapidity, and communicates, in a very short period, its own peculiar smell to the urine. The balsam of copavia has a similar effect, and gives to that fluid a taste that is most intensely bitter.

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* I know one case which occurred in America, where a person swallowed an ounce of nitre, by mistake, in place of Glau-

Bichat long ago ascertained that hydrogen is absorbed in the lungs, and communicates its inflammable properties to the living blood, to such a degree, that the blood taken from a distant vessel produced a flame when a taper was applied to it. Mercury, alcohol, camphor, &c. &c., are partly thrown out of the circulation in the lungs, and communicate their peculiar flavour to the breath. The aërial poisons, which produce fever, opium, acids, &c. &c., darken the colour of the vital current. On the other hand, the whole of the alkaline salts not only redden the blood when taken inter-

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ber's salts. The excitement which it produced was so great that the individual was bled. The blood which flowed from the vein was completely florid, and remained as fluid as if the nitre had been added to it out of the body.

nally, but add to its stimulating power. Now, when active agents are used internally, so as to enter the circulation either by the vena portæ or the thoracic duct; or whether they be absorbed from the skin, or attracted, like the aërial poisons, into the circulation with the air in the lungs; yet, when they mix with the blood they must change its qualities, and give new properties to the whole current. Some active agents enter the circulation, and produce disease, while others mix with the blood, and act as antidotes. Some of these destroy the morbid poisons, remedy their effects, and cure those diseases in the system, which had been produced by an excess of those agents that are deleterious to life.

Mr. John Hunter, who adopted so warmly the doctrine of fluid vitality, and admitted the vital importance of the blood in its healthy state, yet whenever this interfered with his more favourite theory of diseased action in the solids, he forgot, that, if the blood possesses vitality, it must then, like everything else that possesses life, be subject to disease; and, from this very neglect, gifted as he was, his book on the blood has given a false bent to the profession, and done less good, or, perhaps, more evil, than any other that has been published during the last hundred years, unless we except Cullen's 'First Lines of the Practice of Physic,' or the pathological writings of Mons. Broussais. But notwithstanding Mr. Hunter's affection for those tubes that contain and circulate the blood, he has not attempted to conceal facts which might have led even the most determined solidist to infer, that the morbid action in the solids was, frequently, merely an effect of the diseased state of their nutritive fluid.

Mr. Hunter, with all his faults, was faithful in his facts, and when these pressed hard, he sometimes made admissions that are quite in opposition to his own more favourite theory relative to the cause of diseased action. In one part, he admits all that we wish to be admitted, namely, 'that the blood can 'receive and retain extraneous matter, capable of 'destroying the solids, by stimulating to action so as 'to destroy them.' This acknowledgment was drawn from Mr. Hunter, in consequence of the following circumstance. Morgan, a house-painter, who had been paralytic for a considerable time in his hands and legs, died from the effects of an accident, which he survived only three weeks. 'On examining the 'body, after death, it was found that the muscles, 'particularly those of the arms, had lost their natural 'colour; but instead of being ligamentous and semi-'transparent, as happens in common paralysis, they 'were opaque, resembling exactly, in appearance, 'parts steeped in a solution of Goulard's extract. 'From this,' says Mr. Hunter, 'it appears that the 'lead had evidently been carried along with the 'blood, even into the muscles themselves.' Now as the poison, in this case, could only be carried from the lungs or the stomach, through the medium of the blood, to the muscles of the arm, it is very clear, that the poisoned state of the vital fluid had been, in this instance, the sole cause of the morbid action in the distant solids.

But it seems, according to Mr. Hunter's theory, that 'whatever is *dissolved* in the blood must be 'merely diffused through it and not chemically combined with it; otherwise,' says he, 'the nature of 'the blood itself would be altered.' Now we do not hesitate to assert that many foreign agents do chemically combine with the blood and alter its qualities, as completely as they change the qualities of water or any other fluid. But whether they be chemically combined with the blood or not, if they are dissolved and diffused in the circulating current, they must change its physical properties and give it new qualities. It is then altered, it is no longer healthy, and this deranged blood is often the cause of disease, and, in some cases, of organic lesion in particular organs; whilst in others, as in fever, this deranged state of the blood produces diseased action in every fibre of the solids, and causes a deranged state in every one of those secretions that are derived from organs that are now supplied with diseased blood; consequently, when this is deranged, the solids cannot properly perform their functions, and the secreted fluids cannot be healthy.

Many strong facts might be brought forward to prove that poisons are rapidly carried into the circulation, and that this poisoned blood can produce disease and cause death, even in animals that were previously perfectly healthy. In Dr. Christeson's late work on poisons, a case is related of a person who had been poisoned by oxalic acid. Six hours after the poison had been taken, leeches were applied to the stomach, and every one of them were immediately poisoned. 'They were healthy,' says Dr. Arrowsmith, who communicated the case, 'small, 'and fastened immediately. On looking at them, 'in a few minutes, I remarked that they did not 'seem to fill, and on touching one it felt hard, and 'immediately fell off motionless and dead. The 'others were all in the same state; they had all 'bitten, and the marks were conspicuous, but they 'had scarcely drawn any blood.'

A woman was admitted lately at the Meath Hospital, in Dublin, for an obstinate affection of the stomach. 'This patient, at one period of her com-'plaint, was afflicted with constant nausea and vo-'miting, accompanied by considerable tenderness 'in the epigastrium, for the relief of which twelve 'leeches were applied to the region of the stomach. 'Shortly after they had begun to act, they fell off, 'and immediately died. This curious circumstance 'naturally arrested attention, and twelve leeches 'more were ordered, the precaution being first 'taken of perfectly washing the skin, to which they 'were applied. These leeches shared the same fate 'as their predecessors, and this happened in several 'successive trials, made by way of experiment, until 'about sixty leeches, all previously in good health, 'had been thus destroyed; of course, there is no 'other way of accounting for this phenomenon than 'by supposing that this woman's blood was so different in composition from human blood in general, 'that it proved poisonous to these animals *.' This case was under the care of Dr. Graves. The cause of the poisonous condition of the blood was considered doubtful; but the fact is certain, that a short time before the application of the leeches, she had been taking small doses of hydro-cyanic acid. It is true, that in stronger persons larger doses of the same poison may not always produce a similar condition of the blood; for, in them, the deleterious agent may be more rapidly removed by the secreting organs; but, in individuals who are weak, the poison may be retained in the circulation, and contaminate the blood to such a degree, as instantly to kill the leeches that suck the vitiated fluid from the person who had swallowed the noxious agent.

There is something mysterious in the modus operandi of those few poisons that are generally believed to cause death by a direct impression on the brain, made through the medium of the nervous system. The prussic acid, for example, is supposed to produce its deleterious effects entirely in this way; but still it is as suddenly fatal when it mixes with the blood in a vein, as when we inject it into the carotid artery. It is also destructive to vegetables, and to animals, that have neither a brain nor a nervous system. This poison has been detected also in the blood of those

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* See the Medical Gazette, for February, 1831.

who had used it internally; and we know well, that this and other destructive agents could not exist in the vital current without doing mischief. As the nerves possess no power of absorption, it is very evident that all those agents that enter the circulation must produce their effects on the solids chiefly through the medium of their nutritive fluid. Mercury, alcohol, salts, acids, alkalies, camphor, verdigris, sugar of lead, &c. &c., have all been actually detected in the blood, either of men or animals, that had been using these agents, and most of them have been found in the secretions. Now, as the nerves have no tubes, it is very clear that the actual agents could not get either into the blood, or from this into the secretions, merely through the medium of the nervous system. And, in fact, so many of those active agents which cause, and so many of those remedies that cure, disease, evidently enter the circulation and produce their effects on the whole system, through the medium of the blood, that it is difficult to ascertain which of them do not produce their effect, either in part or entirely through the medium of the circulating current, like the air which we breathe, or the chyle which enters the circulation and supplies the blood with materiel for the nourishment of the whole body.

GENERAL OBSERVATIONS ON FEVER.

Without a knowledge of anatomy, surgery is not a science, and without a correct view of the healthy and diseased properties, both of the fluids and solids, even the most eminent physician is but little better than the unskilful surgeon, who is entirely ignorant of the structure of the body; and often the physician, who attends only to the solids, is not more successful in his practice than a common empiric. Unfortunately, however, it has been but too much the fashion, for nearly a century past, to overlook almost entirely the diseased condition of the fluids; yet, notwithstanding this, my belief is, that the importance of attending to the blood, particularly in fever, can scarcely be overrated by the practical physician. I believe also, if ever we arrive at anything like certainty in the treatment of malignant diseases, that this is not only the basis on which we are to build our theory, but the true foundation on which our practice must ultimately rest; and those who neglect this, deprive themselves of their best practical guide: for, as we shall afterwards see, the appearance of the blood is, perhaps, the best index for pointing out the practice that ought to be pursued in the various forms of febrile disease.

In those who are strong and healthy, the blood is rich and florid; when drawn from the body the fibrin coagulates into a solid clot of a dense consistence, and, as we have seen at a certain period, even this, like the living muscular fibres, can be made to contract on the application of salt and other stimuli. In those, however, who are weak, the blood is less florid and thin in consistence; when drawn from the system, and allowed to rest, as it has less vitality, it coagulates faster, but less firmly, and the fibrin forms a thin tremulous inert mass, which is scarcely affected by stimuli, and, in other respects, very unlike the firm florid crassamentum of healthy blood.

No one will deny that the ancient physicians were in fault, for looking only to the fluids as the sole cause of disease; but I believe that those of modern times are equally in error, when they attribute all the ills 'that flesh is heir to' merely to the solids; for the blood is not only endowed with life, and subject to disease, but it consists of nearly the same ingredients as the solids. From these ingredients the organised structures are originally formed, and, during life, they continue to derive their nourishment from the vital current; now, when this is deranged, the solids themselves must inevitably suffer from the diseased state of their nutritive fluid; and in this way the solids, in many instances, literally inherit their diseases from the blood.

As a general rule, fever, or febrile diseases, may be divided into two great classes. First, into those which do not arise from any of the aërial poisons, but depend entirely upon other causes, such, for example, as cold, checked perspiration, long-continued excessive heat, local inflammation, &c. &c. Secondly, into those which do not arise from any of these causes, but are produced entirely by the introduction of some deleterious poison into the system. These two classes are totally separate and distinct from each other; but the last is by far the most important, for it includes not only the many varieties of the marsh fever, but the eruptive, the malignant, and, in fact, the whole of those fevers that are caused by pestilence.

It has been observed, that in some branches of knowledge great errors may be made with little loss to society. An astronomer, for example, may attempt to calculate the exact size of a star, and if he be right, few are the wiser, or if he be wrong, none will be the worse; but the doctrine of fever is decidedly the very foundation of the science of medicine, and any essential error in this may daily be the cause of the death of thousands.

'Fever is a pestilence as deadly in its action as it 'is migratory in its habits. Neither rank nor fortune, 'neither youth nor vigour, can shield from its influ'ence; but the healthy and the young, the helpless 'and the old, the rich and the poor, may alike be its 'victims, *and we can derive no consolation from the* '*belief that this terrible malady is either generally* '*understood or scientifically treated*[*].' This is the honest confession of a solidist of the present day, and it will never be otherwise, so long as they continue to look for the *cause* of fever only in the solids, where it does not exist: for 'surely without a 'competent previous knowledge of the disease, no 'method of cure can be fixed at all; and, in such a 'case, it must be acknowledged that the most able 'physicians are mere adventurers, and unfair ones 'too, in a matter of no less concern than the lives of 'their fellow-creatures. If the patient has the good 'fortune to recover from his illness, and out of their 'hands, it is assuredly more owing to his own 'strength of nature, and happy temperament of body, 'than to their art or advice *.'

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[*] See the Westminster Review for 1829.

'I scorn,' says Warren, 'to speak this with the 'design of giving the least offence to any of the 'faculty, even in its inferior branches, but purely out 'of love to truth, and with a candour becoming an 'honest mind.'

Fever, the most frequent, and the most fatal of all diseases, is decidedly the one that is least understood. The humoral pathologists believed that a diseased state of the fluids was the immediate cause; but, unfortunately, they were ignorant of the properties even of healthy blood, and still more so of the sudden changes which it undergoes in disease. Animal chemistry to them was a fountain sealed up; consequently, both in their theory and practice, they fell into numberless errors; and these are to be regretted, not merely because they shaded, for a time, the doctrines of fever, but also that they induced some of the ablest men that ever adorned the profession to run into an extreme directly the reverse, but equally inconsistent with the phenomena of the disease. In correcting the faults of the humoral pathology, some of these have fallen into errors equally great, if not greater than those who had supported the doctrine of the fluids. In their reasoning on disease *they also look merely to a part*, and, in doing this, they have fallen into a similar error with the ancient writers. Such is the fact; and, perhaps, we may not err when we believe that truth has been probably concealed betwixt these two extremes; for fever, whatever it may be in the beginning, yet, when fully formed, it is decidedly a disease of the whole body.

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* See Warren's Treatise concerning the Malignant Fever in Barbadoes, p. 26.

According to the far-famed theory of Cullen, debility is the first link in the febrile chain; still it is very clear, that if weakness were, in reality, the immediate cause of fever, the disease then ought to begin just where it ends. But, independent of this, we know that debility is so little essential to the commencement of febrile action, that in the climate fever of the West Indies, though this is the most ardent of all fevers, yet there is no debility in the first stage, while the weakness comes on exactly in proportion as the fever abates. According to the same doctrine, spasm in the extreme vessels is the indirect cause of the paroxysm; though in the remitting fevers of all countries there are daily proofs that the fever returns almost immediately after the spasm has been completely removed. We are told also, that the cold is the cause of the hot stage; but, in hot climates, we have often the most unequivocal proofs that this is not the case; for in the climate, or the endemic yellow fever of the West Indies, there is seldom, or ever, even the slightest chill in the commencement. This is, as I shall afterwards endeavour to prove, a fever produced by long-continued high heat acting on the rich blood of the northern strangers who have lately arrived from cold countries. It does not, like the fevers from poison, begin with a cold stage, but commences, at once, with violent increased action in the whole vascular system. This fever is neither a nervous disease nor produced by debility, for those that are nervous and weak are sure to escape, while it chooses for its victims the young, the strong, the robust, and that too at a time when they are newly arrived from northern latitudes, with the 'constitution of spring,' and in the full bloom of health, while their minds are occupied with bright hopes which but too often fade away, and are soon replaced by the stern realities of life. When such persons have been exposed to the remote cause of this fever, they complain of feeling heated, sometimes even before the attack; but, in this disease, there are no other premonitory symptoms; for there are neither previous bad feelings, nor preceding debility. The attack is sudden, and without warning. The first feeling is generally a sudden flush of increased heat, and then commences violent general excitement, such as is never met with in any other fever. This, therefore, is not produced by debility. Neither is this disease the effect of some local inflammation, either in the brain or the gastric organs; for the head is not affected until after the attack, and the stomach is not even irritable for the first twenty-four hours, unless where the patient had been guilty of a debauch immediately before, and, in that case, it would have been irritable whether he had been attacked with fever or not. But independent of the absence of all signs of inflammation, in the first stage, there is no inflammatory crust on the drawn blood—such as we invariably meet with, when inflammation exists in any of the organs; but, in fact, the difficulties that attend all the doctrines of the mere solidists are very great, for most of their inferences are evidently founded on error. In attempting to explain the phenomena of fever, they are not only constantly confounding the effects with the cause, but often they have recourse to explanations which are, in truth, not much better than the doctrines of either fermentation, concoction, or the four humours. We are told, for example, by some of the physicians, who adopted the nervous theory, that spasm, in the extreme vessels, irritates the heart, that debility acts as a stimulus to the vascular system, and that weakness in fever is the cause of strength, &c. &c.

Another party, who were not satisfied with the doctrine of Cullen, had recourse to theories that were even more inconsistent than his. Some of these supposed that man, at his birth, was supplied with a given proportion of what they were pleased to call excitability, which was to last him for life; forgetting that they might, with equal propriety, have asserted that he is, at his birth, supplied with a given quantity of gastric juice, which was to serve him as long as he lived. In supporting this theory they brought forward descriptions that were not very clear, and hard terms that are well calculated to confound everything like common sense. 'The sensorial power of associability,' 'the basis of vital impression,' 'the increased demand in an organ for excitability,' 'the stimulus of necessity,' 'organs losing their vitality from being defrauded of disease,' 'blood-vessels taken by storm,' '*stimulants* exhausting the energy of the nervous system, without any previous excitement,' 'sympathy direct,' and 'sympathy reverse,' were but a few years ago the favourite and cant phrases of the day. Those that supported this doctrine were never at a loss; for if any difficulty did occur, sympathy was always at hand, ready to explain it. If an active agent were taken into the stomach, and a part of it soon after found its way into any of the secretions, why, it got there by sympathy! as if sympathy had tubes to convey nitre from the gastric organs to the bladder, or sulphur from the stomach of a female to the glands of the breast, or from this into the circulation of her child. The nerves, it is true, may convey an impression, which is made by an active agent from one part of the body to another, but, as they have no tubes, it is very clear they cannot convey the specific agent itself either from the gastric organs to the bladder, or from the stomach of a female to the glands of the breast.

When it was proved that the deleterious agents which produce small-pox, the venereal, or any other poison, could be conveyed from the mother to the fœtus in utero, the fact was not doubted; but then the poison got there by sympathy! Sympathy nourished the fœtus, and sympathy made it grow; for, it seems, there is not any direct communication between the mother and the child. Sympathy, or the nerves, must also nourish the body even after birth; for, according to their theory, *all* agents produce their effect by a direct impression on the nervous system; consequently the chyle, and other active agents, which enter with the chyle into the circulation, were of no use, and the quality of the blood was of no importance. Sympathy with them was the sole cause of sickness, and their only agent in its cure; for they reasoned on disease as if there had not been one drop of blood in the living body, and talked about nothing but sympathy, or mysterious impressions made on the brain, through the medium of the nerves. By some of these we are told, that fever is, in all cases, a disease of debility, and to be cured, even in the hot stage, by the most active stimulants. By another of the same class we are gravely informed, that all fevers are produced by one and the same cause, and but mere varieties of the same disease *. In short, that all diseases, however opposite, are one and the same; and that the venereal differs from the plague only in degree. We are told also, that in fever the arteries are sometimes too much stimulated to produce inflammation; but that the two extremes of morbid excitement meet in a point and cause death, without either inflammation, serous effusion, or any other local affection. And the same author, in an awkward attempt to improve their doctrine, cuts the knot, and removes all the difficulties at once with regard to the theory, by telling us that fever is neither more nor less than a case of convulsions in all the arteries *.

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* When Dr. Rush was arguing the unity of disease, he was told by a friend, that he had only one thing more to accomplish, and that was, to find out one remedy which would cure them all.

The belief in such doctrines is now, I believe, very speedily passing away; and wanting, as they all do, the broad and substantial basis of truth to support them, they must fall. And the period is, perhaps, not very distant, when they will only be remembered as a proof that 'whimsical theories creep faster into physic than useful facts.' Yet, however inconsistent the doctrines of Cullen, Brown, Darwin, and Rush may be, when compared with the disease itself, still they have been rapidly followed by others that do not appear to be much better; for the theories of the modern solidists are founded either on mere conjecture, or on premises which we know to be incorrect, whilst many of their assumptions are in direct opposition to the phenomena of the disease. They consider every essential fever as merely symptomatic of some local affection; but they are ill-agreed among themselves with respect to its real habitation. Mons. Broussais, for example, is

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* See Rush's 'Medical Inquiry,' vol. iii., p. 23, &c. &c.

quite sure that fever, in every case, is merely an inflammation of the mucous membrane of the gastric organs; while Dr. Clutterbuck, on the other hand, is equally positive that fever is neither more nor less than the effect of an inflammation in the brain, or in other words, that fever and phrenitis are one and the same. Inflammation, however, is not fever, the causes which produce them are not the same, and we all know that the phenomena as well as the terminations of these two diseases are essentially different. We know also that inflammation often exists without producing fever, and the reverse of this is equally true. In common inflammation, the immediate cause of the disease is a mere increase of action in the larger arteries, produced by irritation, or some local cause exciting re-action in these vessels, by obstructing the circulation in the smaller capillaries; but that inflammation is not the cause of the idiopathic fevers is very evident, for the aërial poisons that produce almost the whole of these diseases act as sedatives, not as stimuli, and these fevers are so far from commencing with inflammatory action, that the first effect of the poison is to paralyze the heart, and this paralysis is, of course, attended with a want of circulation all over the body. In the commencement of inflammation, there is increase of vascular action, redness, and heat; but all this is confined to a particular part, while in the beginning of most fevers, there is a decrease of action in the vascular solids, not merely in a part, but all over the system, for during the first stage the blood scarcely circulates, particularly in the extreme vessels, animal heat is not evolved in its usual quantity, and the whole body becomes deadly cold. The features shrink, the surface becomes pale, and, in some cases, blue or black, as in cholera. In common inflammation there is not in the beginning any want of excitement; the disease bounds off at once into increased action, and remains, for a time, steady in its course; but in every one of the fevers from poison, there is a previous stage of complete torpor, which is generally followed by violent excitement, not merely in a part, but over the whole system.

Common inflammation is, at first, merely a disease of excitement in the vascular solids, which frequently terminates by the formation of purulent matter; but even the most inflammatory fevers seldom terminate in this way. We know also that when the blood is drawn from a patient who is labouring under any inflammatory disease, the fibrin contracts so firmly that the surface is cupped, and this is almost invariably covered with a dense white albuminous crust. But in the African typhus, the blood has no crust in the beginning. This is also the case in the seasoning fever of the West Indies, for though this is the most ardent of all fevers, yet the blood is not cupped, neither is there ever, in this disease, the slightest appearance of inflammatory crust on the surface of the crassamentum. And even if we had no other evidence, this of itself would be quite sufficient to prove how essentially different fever and inflammation are from each other.

Common inflammation is chiefly a disease of the vessels themselves; but in fever there is something more than a mere increase of action in the vascular solids, for in this there is not only diseased action in the vessels, but the blood itself is in a vitiated state. It is evidently deranged in its physical properties; it is not now a healthy stimulus to the vascular organs, in consequence of which there is sometimes a decrease, at others an increase of action in the whole system, and this vitiated state of the blood, accompanied with irregularity in the action of the heart, as well as the great variations from the healthy temperature in the whole body, are, perhaps, the best characteristics in enabling us to distinguish fever from either inflammation or any other disease.

In common inflammation, when we sufficiently reduce the increased action at an early period, the disease subsides and the danger disappears; but in the malignant fevers, even after the excitement is reduced, the greatest danger is still behind; and in the last stage, in place of the lancet, the application of leeches, or the use of irritating drastic purgatives, the patients require permanent stimuli, to keep up the action of the vascular organs.

We shall afterwards see that in those fevers that are produced by the aërial poisons, the blood is vitiated, even before the attack, and this, in the essential fevers, is decidedly the first link in the morbid chain. When we inject a putrid fluid into a vein, we derange the blood, and this vitiated blood produces all the symptoms of putrid fever. Or when we inject a weak solution of tartarised antimony into a vein, this acts as a poison in the blood, and produces, for a time, all the phenomena that occur in the first stage of those fevers that are produced by the aërial poisons. And from what I have seen, my belief is that in all the fevers produced either by marsh miasma, or contagion, the diseased action in the solids is as much the effect of the diseased state of the blood, as it is in those cases where we cause fever by injecting a putrid or a poisoned fluid directly into the circulating current.

In fevers from poison, there is generally a derangement in the gastric organs, such as we produce by injecting an emetic solution into a vein; and during the hot stage of all the inflammatory fevers, there is generally an intense head-ache, produced either by the derangement of the stomach or by pressure on the brain, caused by the increased quantity of blood in the head. But this state of congestion is not inflammation, neither is the sickness at the stomach produced by gastritis; and in some of the most malignant fevers, in cholera, &c., during the cold stage, there is not even one sign of inflammatory action either in the brain or the stomach, for in place of this there is a want of circulation in the whole body. But, besides all this, it is very clear that if Dr. Clutterbuck be right, in referring the cause of fever, in every instance, to the brain, then M. Broussais must be equally wrong in believing that it invariably exists in the stomach or intestines. This great want of unity amongst themselves, the many cases which occur where there is no inflammatory action either in the brain or the gastric organs; but, above all, the complete want of success which has attended the practice founded on these theories, might have led long ago to the belief that there was something radically wrong in the exclusive system of the mere solidists. In this pursuit the labours of thousands have been lost; for nothing has been gained to compensate for either the time or the talents that have been devoted to the subject. Little that is really useful has been added to the theory, and in those violent or malignant fevers where medical assistance is most required, the practice of the solidist is more frequently an evil than a benefit to his patients. But perhaps the very best proof that some radical error exists in all the modern doctrines of fever, is, that for nearly a century past, one theory has followed the other almost as closely as the evanescent spectres in the ghost scene of Macbeth. Like those shadows, they have speedily vanished, and now, with the exception of those who do not think for themselves, and a few enthusiasts, they have left no trace behind, except the conviction that they are all as unsubstantial as the objects that we see in a feverish dream.

It has been said that 'those who come after mount on the shoulders of their predecessors, and discover a more extended horizon;' but in as far as relates to fever, this most assuredly has not been the case. We find, for example, most of the late writers describing the diseased changes of the solids with the most affected degree of minuteness; while the diseased state of the blood, the original and real cause of these changes, is entirely overlooked. But a knowledge of the diseased conditions of the blood is just as important, if not more so, to either the pathologist, or the practical physician, than the morbid anatomy of the mere solids.

Diseased appearances in the organs are certainly more evident to the senses, and more easily described, than those sudden and important changes that occur in the living fluid, either as the immediate cause or the effect of fever. But the contracted system of looking merely to what first strikes the eye has been carried a great deal too far. Water, for example, may contain prussic acid, or some other deadly poison, without any apparent change in its physical properties, yet when this poisoned fluid is injected into a vein, as soon as it mixes with the blood, it destroys its vitality, and death is not the less certain because the water had been but little changed in its apparent properties.

The blood is the pabulum of the solids, and when the whole of their nutritive fluid is deranged, as in fever, there must then be functional disease in every part of the system, and the extent of this is exactly in proportion to the vitiated state of the circulating current. It is also true, that in inflammatory fevers, where the re-action is great, the organs may, and when the disease is not properly treated, they do suffer, in some places more, and in others less, in proportion to their vascularity or connexion. In such cases the brain, the stomach, the liver, and the mesenteric glands, may all be affected, in the first place with functional, and afterwards with organic, disease; and this, in some instances, may be the immediate cause of death. But these morbid changes in the organs are not the cause of fever, for they are as entirely the effect of the diseased state of their nutritive fluid, as irritation in the stomach is the effect of tartarised antimony, when we inject that agent into a vein.

When leeches are applied in the last stage of certain malignant diseases, in some cases they soon become sickly, and in other instances they immediately die. But the same vitiated state of the blood, which causes sickness or death in these cold-blooded creatures, is also, in the human subject, the sole cause of the diseased action in the solid structure; neither is inflammation or increased action a necessary link in the morbid chain which ends in death, for in many of the most fatal cases of malignant fever, there is no excitement sufficient either to produce inflammation or to injure the solids,—there is functional disease, and want of secretion, produced by the vitiated and vapid state of the blood. In some cases there is a diminution rather than any increase of action; even in the first stage, and often there is not one symptom of inflammation during the fatal progress of the disease, nor one inflammatory spot to be seen after death to mark its existence, or induce us to believe, that anything but functional disease had ever existed in any of the solids; yet these are the very cases of all others that are most fatal.

Dr. Rush states, on the authority of Baron Humboldt, that when the yellow fever at Vera Cruz proves fatal in a few hours after the attack, the physicians of that city had endeavoured, in vain, to ascertain by dissection the cause of the death; for those parts which were supposed to be the seat of the disease 'frequently exhibited no marks of inflammation, nor of the least deviation from a healthy state.' Now, if the physicians at Vera Cruz could not discover, in such cases, the cause of death, it was merely because they looked for it only in the solids, where it did not exist. Had they examined the diseased condition of the black and saltless blood, with which the heart is invariably gorged, and had they known how impossible it is for such inert blood to stimulate the heart, or support life, they might easily have accounted for the cause of the sudden death without any reference to the condition of the solids.

The frequent occurrence of such cases gives a death-blow to the modern doctrines of fever, and renders the exclusive system of the solidists altogether untenable. Rush, who foresaw that this would probably be brought forward as a serious objection to his doctrine of fever being produced by convulsions in all the arteries, does not attempt to deny the fact, but he tried to evade it by saying, that 'sometimes in malignant fevers, the two extremes 'of morbid excitement meet in a point and cause 'death, without producing either inflammation, se'rous effusion, or any other local disease.' This, however, is a mere equivocation; for, so far from any increase of force or excess of morbid excitement in the system, there is a want of action from the beginning to the end. Better to have said, at once, that the phenomena in such cases could not be accounted for by any of the modern doctrines of fever, and afford an insurmountable objection even to his own. Or, as he was not ignorant of the fact that the blood is diseased in malignant fevers, he might have used the language of common sense, and stated, that in these cases the vital current was so much vitiated by the action of the poison, even in the beginning, that it was totally incapable of stimulating the heart, so as to keep up the action in the vascular organs, and that the solids cease to act in consequence of the loss of vitality, or the diseased state of that stimulus, the healthy condition of which is essential to life; and when this loses its vitality, or becomes vitiated to a certain extent, the organs cannot perform their functions—consequently the patients must soon die.

To those who look closely at fever, it is evidently no local disease, for its symptoms are universal and peculiar to itself; and those who look merely to inflammation, existing only in some particular organ, as the sole cause, have been obliged to twist their theories into every possible shape, trying to force them to correspond with the various phenomena which occur in fever. But they have neither succeeded in explaining the symptoms, nor advanced us in our knowledge, even in the least; unless, indeed, that to know what a thing is not may be one step towards enabling us to ascertain what it really is.

'Pathological anatomy is but one of the many 'points of view in which we may consider the science 'of disease;' and notwithstanding all that has been said about 'la médecine eclairée par les overtures des cadavrés,' I have a firm belief that morbid anatomy has done little good, particularly in the hands of those who do not understand its real value *; for those who are constantly mistaking the effect for the cause, or confounding the immediate cause of death with the cause of the disease, and forming theories on this foundation, not only deceive themselves, but, unfortunately, particularly for the inhabitants of hot climates, they have deceived others; and those who have trusted to the mere reduction of action in the tropical fevers, and treated the malignant cases with leeches to the stomach, gum-water, and acid ptisans, can best tell the result of their practice in the malignant fevers of the Western World.

Were there no other reason, the want of success which attends the confined views and limited treat-

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* Of course these observations do not apply to those who, like Andral, or my friend Dr. Carswell, study morbid anatomy in the only way in which it can be useful, that is with a reference, not merely to the solids, but to the whole system.

ment of the mere solidist, ought, of itself, to be a sufficient reason for dwelling on this subject, and inquiring into theories that have now become so widely diffused; but when these are examined closely, it is not difficult to discover that most of the arguments which are brought forward in favour of their doctrine, rest on a foundation which is not more stable than that of the rainbow, for those local diseases, which they consider as the essential cause of fever, are so little essential, that often, even in the worst cases, they do not exist: so that this, the assumed fact, which is, in reality, the very basis of pure solidism, is not only incorrect, but even their proof is an error; for the solidists, in the first place, mistake the effect of fever for its cause, and then to add to the confusion, they bring forward that which occasionally produces death, and confound this with the immediate cause of the disease itself.

'Après la mort' (says Monsieur Broussais), 'on trouve toujours des traces de gastro-enterite.' Now, in the first place, this is not true; and, secondly, if it were, it is nothing to the purpose; for when it does exist, it as much an effect of the diseased state of the blood as the chill which is so common in the first stage of the fevers from poison, or the yellow colour of the body, the black vomit, or the petechia in the skin, which are so common in the last stage of the African typhus. But, however common derangement in the stomach may be in the fevers from poison, yet I have seen many cases of the climate fever, where, when the disease was properly treated, the stomach was not even irritable from the beginning to the end; and, in some fatal cases of the African typhus, where the stomach was minutely examined soon after death, both by Dr. Geo. Stedman and myself, there was not even the slightest trace of organic disease either in this organ, the intestines, or, in fact, in any part of the whole body, except in the blood. It was this, and this alone, that was found to be invariably diseased in every case. The above quotation is the very basis on which is founded 'la nouvelle doctrine des fievres,'—a doctrine which has lately been brought forward and supported by a very ingenious and able man. But so far from throwing any light on the theory of fever, it has tended to convert obscurity into darkness; and as to the treatment, if it has a redeeming quality, it is this, that gum-water is infinitely less injurious to the stomach than vitriolic or antimonial emetics, and sugar of lead, as recommended by Jackson; while leeches, in the last stage, are less destructive than the lancet and drastic mercurial preparations, as used by Rush and a number of others; and that, too, at a time, when it is the imperious duty of the physician not to weaken, but to keep up the strength of his patients by proper stimuli.

The practice pursued by some of the solidists is not unlikely to cause inflammation, for they will not give even a Seidlitz powder to remove the irritating secretions from the intestinal canal; but where the disease is properly treated, inflammation, I believe, is a rare occurrence, for often in the West India fevers there is, as I have said, no sign of it during the disease, neither is there any appearance after death to induce us to believe that anything, but functional disease, had ever existed in any of the organs. In some such cases, I have known the disciples of the pathological school pretend to see *something like disease* in the mucous membranes, even when this appearance was invisible to those who had not been in the habit of seeing organic derangement in surfaces where it does not exist; but, unfortunately for those who have supported this theory with so much obstinacy, others have found the same appearances in persons who have died suddenly without fever, and even in animals that have been bled to death. As a last resource, *they* would have us believe that the local disease might have existed, even although they have nothing to show for it, but their own assertion. A cause may be considered as lost when its advocates are obliged to have recourse to such logic as this; and Dr. Clutterbuck might as well talk of inflammation of the brain without head-ache, as the disciples of Mons. Broussais of inflammation of the stomach, in those cases where there is, during the disease, neither irritation, pain, or the least sign of derangement in the gastric organs, nor anything to show for it, after death.

As far back as 1821, Mons. Broussais asserted, that his doctrine would lessen the bills of mortality as much, and even more, than they had been by the discovery of Jenner, a promise which, unfortunately, has not been realized, at least, in as far as relates to the western world; but if those of this school had used Seidlitz powders, or any other mild, saline laxatives, along with their leeches, gum-water, &c., they would have been infinitely more successful in their practice, and the signs of gastro-enterite would have been less frequently met with after death. But from the fear of irritating the stomach and intestines, they allowed the most acrid secretions to remain in the system; and this, of itself, is sufficient to account for the gastro-enterite, which is so common in the practice of those who have adopted the doctrine and followed the treatment of the pathological school.

Whatever the remote cause of fever may be, it is very evident that this cause must invariably exist, not only at the moment of attack, but even previous to that period. Now, in the fevers from poison, the blood is invariably diseased previous to the commencement of the cold stage. During this period there are premonitory symptoms, but these are evidently the effect of the diseased state of the vital fluid; and that these precursors of fever are not the effect of any local inflammatory disease is evident from the fact, that frequently during this period there is no pain in any of the organs, but a want of action, particularly in the extreme vessels, and, consequently, a decrease of heat in the whole system.

If inflammation in any of the organs were, in reality, the cause of fever, then the disease ought to be fatal, exactly in proportion to the violence of the local affection; but the very reverse of this is the truth. Mere excitement can easily be reduced, and the inflammatory form of fever is decidedly the most easily cured, though in it the excitement is often so great that the organs are very liable to be injured; while the malignant form of fever is by far the most fatal, though in this the excitement is less, and the organs are seldom affected. This is particularly the case in the worst form of the African typhus, in cholera, the plague, and probably in all other varieties of malignant fever, where the blood is under the influence of an active poison, and where its vitality is diminished and its structure is injured even before the attack.

Those who have seen most of the malignant diseases know well that excitement in fever is invariably a good symptom; for this is a sure sign that the blood has not yet undergone any fatal change, and, independent of this, mere increase of action is always at the mercy of the lancet. But neither the lancet, nor leeches, gum-water, vitriolic emetics, calomel, antimony, brandy, opium, or acids, can redden the colour of the black blood which we invariably meet with in pestilential diseases, or remedy the diseased state of the vital current, so as to cure that fatal form of fever where the malignant symptoms are produced, not by excitement, but by the vitiated state of that mysterious fluid, which, in health, gives life and nourishment to every solid of the system, and which, when diseased to a certain extent, is by far the most frequent cause of death in all those fevers that are produced by some deleterious poison acting, in the first place, on the vital current, and then on the brain and the whole system, through the medium of the blood.

In those malignant cases, where the poison has been applied in its most concentrated form, or where the person is weak, the blood is so much vitiated, even early in the disease, that it loses the power of stimulating the heart, so as to keep up its healthy action; and, perhaps, also the vascular organs are early affected by the action of the poison, and lose the power of either feeling the stimulus or re-acting with force on the impression which is communicated to their internal surface by the vitiated blood. In such cases, in place of increased excitement, there is frequently a want of action in the whole system. This diseased state of the vital fluid is the effect of the remote cause acting on the vital current, but particularly by immediately lessening its vitality, and ultimately diminishing the quantity of its saline ingredients. And we know that the diseased blood, which has almost entirely lost its saline matter, as in the last stage of fever, can neither stimulate the heart, evolve heat, nor support life. Now, if this poisoned or deranged state of the blood be the cause of the disease, and its further vitiation be but too often the cause of death, is it not then evident, that the physician who overlooks this can neither do his duty to his patient, nor expect to be generally successful in his practice?

The African typhus commences with a cold stage, which, in general, is soon followed by violent excitement all over the system; but I have seen some fatal cases of this disease, where, from first to last, there was no re-action, either in a part or in the general system. In such cases there was sudden and great debility without any apparent cause, and great real without any apparent danger; the patients were restless and uneasy, yet they were so free from pain, that the friends could scarcely be persuaded that they were seriously ill. They believed that where there was no increase of heat, there could be no fever, and where there was neither fever nor pain, there could be no danger, but in this they were soon undeceived by the fatal result.

A common inflammatory fever may frequently be cured by the mere reduction of increased action; but it is in the malignant fevers a fatal mistake to attend only to the solids, for these may be easily protected during the hot stage; yet when this is accomplished in such fevers, the most formidable danger is still behind: for in such diseases it is only by attending to the vitiated state of the blood that we can ever prevent the fatal result; and the total neglect of this has been, as I believe, the chief cause of their great mortality.

When we look on fever as a disease of the solids, we are constantly at a loss to understand the symptoms; but when we consider fever as a disease of the blood, almost all the phenomena explain themselves; and, what is more to the purpose, with this belief, even the most malignant cases are much more easily cured. But false theories in medicine lead to the greatest errors in practice; and if the mere solidist ever do any good in fever after the first stage, it is only by chance, whilst in all the malignant cases he is completely at fault even in the beginning, for he looks only to the solids, where the cause of the evil does not exist; he is a mere prescriber for symptoms, who never strikes at the root of the evil, for he overlooks entirely the cause of death, and treats the diseases of the living body, as if he had only to contend with a mere inanimate machine. Whether the poisoned state of the blood be or be not the immediate cause of the malignant fevers, yet, if it be a fact, that the vitiated and saltless state of the blood is, in these diseases, the real source of the bad symptoms, and but too often the sole cause of mortality,—and if this diseased state of the vital current be produced by changes which take place either in the commencement or during the course of the disease,—it must then follow that those who attend only to the reduction of the excitement in the solids, are guilty of a great error in practice; for by attending merely to the solids, and neglecting entirely those fatal changes in the fluids, they leave undone by far the most important part of their duty. They reduce the excitement, and that too often at a time when it is no longer either necessary or proper, yet the disease continues they know not why; their patients die, and that too at a time when those who attend to the solids cannot assign any cause for the sudden death, but console themselves with the belief, that in their patients the disease had been uncommonly malignant.

Dr. Rush fell into most of the errors of the modern schools; but yet, with all his faults, he was no commonplace character. He was a warm enthusiast in his profession, and many of the facts which he has left on record are of infinite value. After having witnessed the unsuccessful results of the various modes of treatment adopted by the solidists, in malignant diseases, and even of his own two boasted specifics, the lancet and calomel purgatives, he came at last to this sad conclusion:—' But what have ' physicians, what have universities or medical so' cieties done, after the labours and studies of many ' centuries towards lessening the mortality of pesti' lential diseases? They have either copied or con' tradicted each other in all their publications; for ' plagues and malignant fevers are still leagued with ' war and famine, in their ravages upon human life.'

Rush, as I have said, was a complete solidist; but with all his fondness for their theories, he was at last so little satisfied with the result of their practice in pestilential diseases, that he even argues the propriety of leaving the patients entirely to themselves. ' The history,' he observes, ' of the yellow fever in ' the West Indies proves the advantage of leaving 'patients to their own judgment. Dr. Lind has 'remarked, that a greater proportion of sailors, who 'had no physicians, recovered from that fever, than 'of those who had the best medical assistance. The 'fresh air on the deck of a ship, a purge of salt 'water, and the free use of cold water, probably 'triumphed here over the cordial juleps of physi-'cians.' This I believe to be most religiously true, and afterwards I may bring forward many facts to prove that this opinion is but too correct. Such has been the melancholy result of the modern and unphilosophical system by which we have been taught to look merely to a part of the living system, and to consider the most important of the whole as of no value in the treatment of disease.

With all its imperfections, I believe that the first doctrine of fever came nearest to the truth. It was the doctrine of observation founded on common sense, and is that which is most in accord with the phenomena of the disease. The studies of the ancients consisted in observing nature and recording facts; but as animal chemistry to them was a fountain sealed up, and ignorant as they were of the properties even of the healthy blood, it is not to be wondered that they fell into many errors. But still the old doctrine contained the wisdom of many ages, and stood its ground for a much longer period than any of the others that have since followed; and, perhaps, it would have been well for the world, if the reformers in medicine had imitated those who improved our religion, and pruned, like them, their tree of knowledge, in place of tearing it up by the very roots. Experience, however, has now taught us not only the nature but the result of the error, and we trust that, ere long, the lesson which we have received will not be lost.

Until about the beginning of the last century, the small-pox, the measles, and scarlatina were universally considered as so many varieties of the same disease; and, even to this day a similar error exists with respect to the three essential fevers that are generally met with in the West India Islands. One of these is the climate or seasoning fever. This is not produced either by the marsh poison or contagion, but is caused by long-continued excessive heat, acting, under peculiar circumstances, on the bodies of unseasoned strangers, who have lately arrived from northern countries. The second is the marsh, or swamp-fever, in all its varieties. This disease is met with in every locality all over the world, where there is a large surface of stagnant water exposed to the action of a hot sun. The third is the African typhus, or the pestilential fever of Chisholm, the Bulam fever of Sir William Pym, and the yellow fever of a variety of writers. The two former are indigenous to the West Indies, and so far as I have seen them, they are never communicable from one person to another; but the last is, as I believe, in every instance, a contagious disease. At one period, there was an interval of thirty years, during which there was not even one case of this fever in the western world; and in almost every instance, where it first commences, when proper inquiry is made to ascertain its cause, its origin can be traced to the traffic with certain countries on the western coast of Africa.

About the time that I first went to the West Indies, I read the works of Hunter, Bancroft, Mosely, and others, whose writings were most in repute, on the diseases of hot climates; and in consequence of the opinions I had imbibed from these authors, I believed then, and long after, that contagion, as connected with the West India fevers, existed only in the imagination of a few mistaken individuals; and of this I was so perfectly satisfied, that, like most others, I took no trouble to ascertain the truth by my own personal observations. I have since, however, seen quite enough to convince even the greatest sceptic, that contagion, as connected with one at least of these fevers, is neither a phantom of fear, nor 'the shadow of a shade,' but a stern, and too often, a fatal reality. The climate or seasoning fever is never contagious, neither are the different varieties of the marsh remittent; but, in the African typhus, as well as in that new disease, the dandy fever, we must either shut our eyes against the most positive evidence, or admit that contagion is the sole and the only cause of both these fevers; for the proofs which I have seen, in favour of this, are in my mind just as strong as those in favour of the contagious character of either small-pox, scarlatina, or any other contagious disease.

The climate fever is only met with as an epidemic during the hot months, when the thermometer is upwards of 88° during the day, and at least 80° during the night. It occurs but very seldom in swampy districts; it appears generally in dry situations, and only in solitary cases, except in those localities, where there is an accumulation of unseasoned strangers, exposed to the action of a burning sun. This disease is confined to the whites, and almost entirely to those who have arrived lately from northern countries. But the African typhus appears in the West India islands in every locality, and at all seasons of the year. It is not confined either to the whites, or to those who have lately arrived. I have known it fatal even to the negroes as well as creoles, who had never been out of the tropics. And where it does exist, in the West Indies, it is just as fatal in the coolest weather as it is in the hottest months. This disease is not produced by any local endemic cause; and where it makes its appearance in any of the islands for the first time, there is but one thing that is invariably essential to its existence, namely, an arrival from some sickly port, where the same disease had existed previously to the sailing of the vessel. After a minute inquiry into this subject, I may venture to say that there has not been even one solitary exception to this rule, either in the West Indies, or in the United States of America. And, afterwards, I may bring forward many unequivocal facts, to prove that in places where the above cause has existed, the African typhus has made its appearance in the most cleanly and healthy situations, where the same disease had never existed, until a few days after the arrival of an infected vessel.

The marsh fever is seldom or never met with as an endemic, in the centre, or near to the wharfs of large, well-built cities; while the African typhus and other contagious diseases are most fatal where there is a dense population; and the first cases of this disease, almost invariably, occur about the wharfs; or in that spot where the strangers from abroad first come into contact with the natives. When a number of individuals are equally exposed to the marsh poison, many are attacked about the same period; but the African typhus always begins with solitary cases, and often there is a considerable interval betwixt the primary cases and those that occur afterwards; but when these once begin, the disease marches on with a steady pace, from the spot where it had commenced towards the interior parts of the town.

When the poison, which produces the African typhus is first evolved from the body, it appears to be almost inert; but, by being confined in clothes or bedding for a short time, without being exposed either to the air or the sun, it undergoes changes, and becomes so virulent as to produce even the worst form of the disease, in those who are exposed to the fomites, which contain the poison, when the infected clothes are first exposed to the atmospheric air. A sailor, for example, may be attacked with this disease in the West Indies, and, during his illness, other individuals, even those who have not had the disease, may visit him while sick with the most perfect impunity; but, if the clothes which this sailor has worn, during his illness, are packed up unwashed and confined in a chest or a bag, and carried, for example, from the West Indies to the United States, during the hot months, when the contaminated clothes are first exposed, the poison is evolved by the heat, or attracted by the air, and those who are near it are sure to be attacked with the very worst form of the same fever.

In large cities there is frequently great difficulty in tracing the origin of contagious diseases to their original cause; but, in small towns or villages, the facts are occasionally so evident, that little doubt can exist with respect to the origin of the fever. A few years ago, a well-marked instance of this kind occurred at the small town of Bristol, which is built on the clean and healthy banks of the Delaware, about eighteen miles higher up the river than the city of Philadelphia. This small town, or rather village, is remarkably healthy; but in the month of July, 1825, the African typhus, or as they called it, the yellow fever, broke out in one house in the outskirts of the village, and out of eleven persons, seven were attacked, and three died, after a short illness, with the yellow skin, the sudden cessation of secretion in the kidneys, and the black vomit. On inquiry, it was found that the son of the proprietor, who is captain of a West India vessel, had arrived immediately before, and had brought with him a bag containing shirts, and other unclean clothes which he had worn while he had had the yellow fever in the West Indies. This bag was opened, and the clothes were washed in the house, and the same disease which the brother had had, broke out in the family a few days after the exposure of the infected clothes to the air. Now as this occurred in one of the most cleanly and healthy spots in the United States, there can be little doubt about the cause of the fever, not only at Bristol, but in many similar instances, both in the West Indies and in that country.

Those who have once had the climate or seasoning fever are not susceptible of it a second time, unless they leave the West Indies and return again, after a considerable residence in some northern country. Those who reside in swampy situations generally escape this disease, for such places are not only more cool, but besides this, those who reside near to marshes are generally soon attacked with the remitting fever, and by repeated paroxysms of this, the system is changed without an attack of the seasoning fever. The marsh poison, however, does not give an exemption from that which produces the African typhus,—neither does an attack of the climate fever; but those who have once had the African typhus are completely exempt from any future attack of the same disease. When they are afterwards exposed to the marsh poison, this produces its effect, both in those who have had the climate fever, and in those who have had the African typhus; and, by many, these subsequent visitations have been considered as second, third, and fourth attacks of the yellow fever. But all this is merely one of the many evils which have arisen from the unitarian system of grouping several diseases into one, for those who believe that the climate fever and the African typhus are only the more aggravated forms of the marsh remittent, may bring forward twenty successive attacks of the yellow fever; for though most individuals, after a long residence in a swampy country, may, at last, become seasoned to the poison, yet there are others, particularly those of a scrofulous habit, who are subject to the marsh fever, almost every time that they are exposed to the action of the remote cause. But that the climate fever is not produced by the marsh poison is evident, not merely from the symptoms, but also from the fact, that it is generally met with in hot and dry situations, such as the central part of the town of St. Thomas, where the marsh fever is not known as an endemic; while in the eastern and western districts of the same town, which are near to swamps, the inhabitants are very subject to the marsh fever, but almost entirely exempt from that more violent disease, which, during the hot months, is so common and fatal to strangers in the central and dry part of the town.

The above three fevers are totally separate and distinct from each other, and easily distinguished, particularly in the beginning. They are produced by different causes, they have symptoms that are peculiar to each; and though in the outline the treatment in all may be nearly the same, yet the fact is that opium, acids, and other remedies, which may be used, not only with safety but even with advantage in the marsh fever, cause almost certain death, when given to any extent, particularly in the last stage of either the climate fever or the African typhus.

However separate and distinct these fevers may be from each other, to those who have observed them closely, yet the truth is, that a great majority of the West India writers have grouped them all into one, and described the whole as a protean disease, under the common name of the yellow fever. Now, it is this improper system of grouping so many diseases into one, that has been, and still is, the source of so much confusion with respect to the remote cause of the yellow fever. It is this that has given origin to so many arguments about the propriety of using certain remedies in the treatment. It is this also that has been the chief cause of the long and unsatisfactory discussions which we have had upon the subject of contagion; for those who have seen only the climate or the marsh fevers, believe that there is no contagion in the yellow fever, while Chisholm and the other physicians, who were present in the West Indies when the African typhus, after a long absence, made its re-appearance in 1793, believed, almost to a man, that that fever was both a new and a contagious disease. But the fact is, that two of the above fevers are never contagious under any circumstances, while the third is, as I believe, invariably the effect of a contagious poison, and cannot be produced by any other cause. Yet I know more authors than one who have returned to England, written largely on the yellow fever, and denied contagion, though they had never seen a single case of the African typhus, for this disease did not exist during their short residence in the West Indies.

I may afterwards bring forward many facts to prove the correctness of the opinion, that those who have once had the African typhus are completely exempt from any future attack of the same disease: this, however, has been doubted, particularly by those who do not remain long enough in the West Indies to ascertain the fact by their own personal observation. When this disease re-appeared in St. Croix, in 1817, after an absence of eight or ten years, it did not confine itself to those who had lately arrived from northern climates, for it committed its ravages, not only on strangers, but on natives, on young children, and, on some estates, even the negroes suffered severely, while the only individuals who completely escaped were the old inhabitants, who had had the disease, during some of its former visitations. The epidemic of 1817 continued about six months: during this period, those that were susceptible and exposed to the poison were all taken ill; of these a great proportion died, and those that recovered were exempt from a second attack; consequently the disease soon ceased as an epidemic. After this it was of course confined only to strangers; for when these arrived and went to reside in houses where the disease had been, or, perhaps, slept on the same mattress which had been used by those who had previously had the disease, they were soon attacked with the same fever, without being at all aware how or when they had been exposed to the cause. Now physicians who arrive under such circumstances, and observe the disease confined only to strangers, naturally conclude that this fever cannot be contagious. But this, which is one of their chief arguments, is no proof ot its non-contagious character, and on the same ground they might deny the contagious nature of the small-pox; for when this disease has once fairly gone through any of the West India islands, it is sometimes kept up in the same way, and confined only to strangers who arrive from other parts, and have not previously been vaccinated, or had the small-pox in their own country:—but of this hereafter.

That peculiarity of constitution which is necessary to enable the natives of northern countries to resist the effects of extreme cold, is very different from that which is required to enable the inhabitants of hot climates to resist the effects of long-continued excessive heat. The inhabitants of cold countries are generally surrounded by a more bracing atmosphere than those who reside within the tropics. Their digestive organs are more vigorous, their blood is more rich; it is more dense, for it contains a larger proportion of solid ingredients. That in the arteries is more florid, for it contains a larger proportion of those salts which are, in reality, the true cause of the arterial colour of the blood; and though, with the exception of its vital principle, these are the ingredients of all others the most essential to the healthy state of the circulating current, yet as these add to its stimulating power, it is probable that this excess of saline matter is the chief cause why the blood of the northern stranger is more inflammatory and stimulating than than that of the creole, or even the European, who has been seasoned to the tropics.

A sudden change of climate requires a change in the whole system; and the young northern stranger who arrives and resides in hot dry situations on the burning shores of the West India Islands, with a strong robust constitution and rich stimulating blood, is constantly on the very brink of a precipice, until his system be changed. This is sometimes slowly effected by residence alone; but, in general, this necessary change is suddenly produced by an awful disease.

It is a common belief that heat, however excessive, cannot produce fever; this is often, but not always, true. We have seen that the animal heat is evolved in the extreme texture all over the system, but the skin has a considerable share in regulating the temperature of the body. When we are surrounded by a medium that is considerably lower than 98°, the excretory vessels of the skin are then closely constricted, and from the non-conducting power which the skin possesses, it prevents the equilibrium and retains the heat in the system, in the same way that a glove retains the heat of the hand. But under ordinary circumstances, when we are surrounded by an external heat which is much higher than that of the blood, the action of the minute arteries of the skin is immediately increased, a thin serous fluid is thrown out on the whole surface, and the evaporation of this serves as a cooling process, which prevents the heat both of the blood and the solids from rising higher than their natural temperature of 98°.

So long as the skin performs it natural functions, the greatest heat, even in hot climates, cannot produce fever. But the living blood is infinitely more delicate and much more easily affected by extreme heat, cold, &c., than any of the solids; and when a young northern stranger is exposed, soon after his first arrival in the West Indies, to a high temperature, particularly in a warm room, his clothes are immediately drenched. And if, while in this state, he be suddenly exposed to a free current of cold night air, the vessels of the skin are suddenly constricted, the perspiration is instantly checked, and we all know that when the exhalent vessels are constricted in this sudden and forcible manner, they are not again so easily opened. If the individual who has been thus exposed remain in a cool situation, he may perchance escape sickness, or, if he be attacked, it is generally a mild case of the climate fever, a disease which, in its mildest form, resembles the common inflammatory fever of northern countries; and as the other secretions are also diminished, it is probable that this fever is produced by derangement of the blood, caused by the retention of those acrid fluids in the circulation which ought to have been removed by the secreting organs. But, in hot climates, when an unseasoned stranger is exposed, with a constricted skin, for some hours, to the direct rays of the sun, in a temperature that is, perhaps, twenty degrees higher than the natural heat of the body, this high external heat acts on the circulating current in the same way that it does on mercury in the thermometer; for now that the cooling process of perspiration is no longer kept up, the high heat raises the temperature of the blood, and renders it not only more fluid, but augments its volume. This increase of volume in the blood produces unnatural distention, not only in the heart, but in all the vessels, and this of itself causes reaction in the whole of the vascular solids; but independent of this, it is the blood which is the cause of the motion of the heart, and the extent to which it produces this effect depends almost entirely on the degree of its stimulating quality; the increase of heat not only renders the blood more fluid, but it adds greatly to its power of stimulating the vascular organs. When the excitement is fairly commenced the process for evolving animal heat is carried on with great rapidity in the extreme circulation, and when the temperature of the chest is so high as it generally is in this fever, the blood is rapidly purified in the lungs. This increase in the arterial quality, together with the increase of heat, adds so much to the stimulating power of the whole current, that the impression which is made by this hot and stimulating blood, on the heart and vessels, causes such violent excitement, that this is decidedly the most ardent of all fevers, and one that requires the most active means to arrest its progress.

The climate or seasoning fever of the West Indies can easily be distinguished from the African typhus, or any of the other fevers from poison; for in this disease, there are no marked premonitory symptoms, there is no cold stage in the commencement, there is no foulness of the tongue, no sickness, or even irritation in the stomach, at least for the first twelve hours after the attack, there is no derangement in the biliary organs, no spasms in the gastrocnemii muscles. All the secretions are diminished, but there is no redness in the fluid which is secreted in the kidneys, whilst the pulse in the first stage is not only incompressible, but the artery at the wrist is distended to a degree which is never met with in any other disease.

In the beginning of the West India fevers, the one, however, can be distinguished from the other, better, perhaps, by the appearance of the blood, than by the symptoms which exist in any of the solids. In its first stage, the climate-fever is a disease of violent excitement; and though, during this period, the increased action is, perhaps, greater than in any other disease, yet the hot blood that is now drawn is never cupped, neither has it the slightest appearance of a crust on the surface. It flows from the vein with great force, and even in the first stage it is evidently diseased. It is not only deranged by the high heat and rapid motion, but also by the retention of those salts, &c. which ought to have been removed by the secreting organs. When this hot blood is drawn from the body early in the disease, it is so florid, that

I have seen young practitioners who were afraid that they had opened an artery in place of a vein. The fibrin coagulates firmly; but, as I have said, there is no crust on the surface of the crassamentum, and in some cases, even the serum when it separates, has a bright arterial colour, which, in this disease, it retains for days; for the colouring matter is not merely diffused but combined with the serum, in such a manner, that it cannot be separated either by filtration or any other mechanical process.

In the last stage of the same fever, the blood is invariably black; for at this period of the disease its saline ingredients are so much diminished, that they are no longer capable of giving a red colour to the hæmatosine, and the blood, even in the arteries, is not only dark, but to use the words of an old writer, the whole of 'the sprightly, vivifying fluid, becomes a 'lazy, stagnating, inactive puddle*;' in consequence of which the action of the heart is greatly diminished, the secreting organs cease to secrete, and the functions of the whole animal economy are completely perverted. In severe cases of this fever, the blood, during the disease, loses also a large proportion of the fibrin, the albumen, &c., and as these are the cause of its consistence, it becomes so thin that it can scarcely be retained in its own vessels, and long before death it oozes from the tongue, the eyes, and other places where there is not even the slightest abrasion of surface. In this stage of the disease, the

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[*] See Towne, p. 54.

saline ingredients which, in health, are the cause of its red colour, are so much diminished, that the blood, in many cases, becomes, literally speaking, as black as ink; and, as the natural salts are the ingredients of all others the most essential in enabling the blood to stimulate the heart, &c., when it loses these, it becomes so vapid, that this black and saltless blood is totally incapable of producing a sufficient impression either on the heart or the vessels, so as to enable them to keep up their action, and the patient dies, not from organic disease in any of the organs, but from the inanimate, saltless, and vapid state of the black blood, which renders it totally incapable of supporting life.

It has been supposed that this blackness in the blood is produced during the last moments of existence, merely from the want of its proper purification in the pulmonary organs; but that this is not the case is evident from the fact that the blood is black, long even before death, and previous to any impeded action in the lungs. But independent of this, blood that is black, merely from an excess of its venous quality, becomes instantly red on exposure to the air; but the black blood which we find in the heart after death, from either the climate-fever or the African-typhus, remains black, even in an atmosphere of pure oxygen. Neither can we redden distilled water with this blood; but it instantly changes colour when we add it to a clear fluid that contains even a small portion of any neutral salt, for the black hæmatosine of diseased blood is so delicate a test for saline matter, that it instantly reddens the colour of any saline fluid, even when the salt is not perceptible to the taste.

When we coagulate the albumen and evaporate the serosity of the blood, which is drawn during the last stage of the climate fever, we find that the saline matter is almost entirely lost, and of this there is scarcely a trace left in the black blood, which is taken from the heart after death. Now, how are we to account for this? for in this disease there is no poison in the blood, either to interfere with the agency or decompose the saline ingredients. There is no bowel complaint as in cholera, to drain off the serous or saline portion of the circulating current, yet at a certain period of the climate fever, the alkaline salts are diminished or decomposed, and disappear as certainly, and almost as suddenly as in those that are killed by the electric fluid; but of this hereafter.

It has been ascertained lately by Dr. Stoker, Dr. Burne, Dr. Tweedie, and other intelligent writers of the present day, that the blood is decidedly deranged, even in the milder fevers of northern climates. But, it is no new discovery that the blood is diseased, both in the ardent and malignant fevers of hot climates; for, this fact has been long known, and faithfully described, by Towne, Warren, Hume, Hillary, and others of the older writers on the diseases of the West Indies. Unfortunately, however, the modern practitioners in hot climates have attended too little to this, while the older physicians knew nothing either of the healthy properties of the blood, or the cause of its sudden dissolution in disease, and still less of the proper means for preventing those fatal changes in the vital fluid, which were the chief cause of the sudden death which occurred so often in those fevers which they had to contend with.

Dr. Hume, who, about a century ago, practised many years in Jamaica, and had charge of the naval hospital in that island at a time when there were large fleets in the West Indies, observes that '*about* '*the end of the third, or beginning of the fourth day* '*from the first attack, the texture of the blood comes* '*to be so broken down and dissolved as to be capable* '*of entering the smallest vessels; and oozes out* '*from the nose, mouth, ears, eyes, anus, and even by* '*the skin, where blisters have been recently applied.* 'About this time, the pulse sinks, flutters, and sometimes intermits; the patient becomes comatose, and 'at intervals, delirious. Starting and universal tremors succeed,' &c. &c.

'When I speak of the several periods or changes 'of the disease, as happening at such and such particular times, I would not be understood to mean 'that they always fall out at the precise times I 'have allotted for their appearance; as when I say, '"about the close of the third, or the beginning of 'the fourth day, from the first attack of the disorder, 'the texture of the blood comes to be so broken 'down," &c., I do not mean to assert that this 'change always happens at these particular times; 'for, in truth, it does not, but sometimes it comes 'sooner than the earliest of these periods, and at 'other times later than the last, and so of the rest; 'my intention being to represent things in their 'ordinary or most common course.'

When we overlook the hard words of which most of the older writers were so fond, and lay aside the antiquated idea about the bile being the immediate cause of fever, there is infinitely more truth in the following description, than in many of those that are more modern. 'From an attentive consideration 'of all the symptoms which attend this disease, and 'a strict examination of the putrid state, and dissolved gangrenescent condition in which we find 'the blood of those who labour under it, as well as 'the half putrefied and mortified state in which the 'body is found immediately after death,—whether 'this fever proceeds from infectious miasmata, or it 'arises from the great heat of the air and water, and 'the putrefaction of our fluids, &c., from thence, and 'is thereby indigenous to those countries which are 'situated within the torrid zone, or whatever is its 'procatarctic cause,—it evidently appears from all the 'symptoms which attend it, as well as from their 'putrid effects, that a bilious putrefying diathesis is 'actually introduced into the blood, and all the circulating fluids of the body, whereby not only the first 'and second concoctions, or the *chylification and 'sanguification of the blood*, are so disturbed, 'altered, and changed, that all the humours, and 'particularly the bile, are by the rapid motion of the 'blood, and greatly increased heat of the body, so 'inquinated with a putrid, bilious acrimony, which 'in a little time so attenuates and dissolves the texture of the blood, that it runs off by the various 'excretory passages and the pores, but also errores 'loci fluidorum are produced, whence the brain is 'affected, and all the animal functions so disturbed 'and altered, and the texture of the blood is so dissolved, that all the humours of the body are almost 'changed into a putrescent lethiferous ichor, which '(*if not timely prevented*) must inevitably end in 'death.'*

The deranged condition of the blood, which produces fever, is decidedly the effect of the remote cause, while this thin and dissolved state of the vital current is evidently the effect of the disease; but the saltless and starved state of the vital fluid is too often, in these fevers, the sole cause of death. In the inflammatory stage of the climate fever, the blood is deranged by the heat, &c., but in the first stage, it is never dissolved; consequently, this change which occurs during the disease, however frequently it may be the cause of the mortality, cannot have any effect as the cause of the febrile excitement; and so far is this loss of the solid ingredients of the blood from being the cause of fever, that, in favourable cases, the patients begin to recover just at the moment when the blood is most thin; and even in the most ardent and fatal fevers, the excitement uniformly abates exactly in proportion as the blood undergoes these changes: while, in some of the worst cases of the fevers from poison, where the vitality of the blood is nearly destroyed even early in the disease, there is then but very little increased excitement. In some cases, there is no hot stage; and in those that are most fatal, there is a want of action from the commencement of the attack up to the last moment, when the disease terminates in death.

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* See a Treatise on the Putrid Bilious Fever, commonly called Yellow Fever, by William Hillary, M.D.

It is very evident, even at first sight, that the climate or seasoning fever of the West Indies is neither produced by debility nor a nervous disease, for the young and the strong northern strangers, who arrive within the tropics 'fearless and full of life,' are the most likely to suffer; but in all the fevers from poison, those who are weakest are most liable to be attacked; for in these, the vital or preservative power of the system is least able to resist the effects of the morbid agent. The climate fever, however, is not the effect of a febrile poison. It is only met with in dry situations, and in the hottest weather; its victims are the young unseasoned strangers, 'its parent is the bright God who governs the day.' I have stated that there are no premonitory symptoms, nor any cold stage in the beginning of this fever; there is no foulness in the tongue, no redness in the urine, *no derangement in the liver, nor irritation at the stomach.* The pulse is full and incompressible, but not so frequent as it is in the fevers from poison. The blood, in the first stage, is hot, fluid, and when drawn from the body, it is infinitely more florid than the blood of health. When the fibrin becomes solid, it coagulates firmly, but there is never even the slightest appearance of crust upon the surface of the crassamentum; and when the serum separates, even this, in many cases, has a bright scarlet colour. But in those who are under the influence of the marsh poison, the attack is preceded with premonitory symptoms, and begins with a cold stage; the tongue is foul, the stomach is irritable, the liver is deranged, and the epigastric region is so tender, that the patients can scarcely bear it to be touched, there is frequent vomiting of a bilious fluid, the urine is scanty and high coloured, and the blood itself is both black and diseased, *even before the attack*. During the cold stage it is very dark. In the hot stage it becomes more red, and in some cases, it is even florid for a time; but during the remission it is darker in colour than the blood of health, and decidedly diseased in all its properties. In milder cases, the blood which is drawn may coagulate without a crust on the surface. But I have seen in the more severe forms of this fever, where, when the blood was drawn at an advanced period of the disease, a part of the albumen coagulated on the surface of the fibrin, and formed a diseased mass, which, in appearance, had a greater resemblance to oatmeal gruel, than to blood which is drawn from a healthy person. The serum which separated was also diseased; it had a brownish colour, and in some cases, an oily appearance, which is never met with in the clear serum of healthy blood.

The Genesee country of the United States of America is situated in latitudes 42° and 43° north. It extends westward from Utica to the river Niagara. It is bounded on the north by lake Ontario, and on the south by the Alleghany mountains. The surface of this extensive district is so remarkably flat, that the Erie canal passes through it for seventy miles, without even a single lock; and in fact, it might have been carried on the same level, all the way from Utica to Lockport, that is, for a distance of two hundred miles. This is the lake country of the United States; but the rivers which conduct the waters from the numerous smaller lakes to the Ontario are almost stagnant. In most of the districts in this country there are extensive swamps, and, during the hot months, there are certain localities in this territory where the marsh fever is as prevalent as it is in Sierra Leone, or, perhaps, in any other part of the world.

The marsh fever is the prevalent endemic of the Genesee country, and those who are exposed to the poison are generally attacked with the most aggravated form of this disease, such as we occasionally observe it in the most swampy situations in the West Indies; but even in the worst cases it has no more resemblance to either the climate fever or the African typhus, than it has to the plague of Egypt, or the Indian cholera.

The sickly season in the Genesee country generally begins about the middle of July, it ceases on the approach of the cold weather, in October, and during the winter, the inhabitants are exempt from the marsh fever. But in the hot months, this disease again appears, and in many districts the cases are so frequent, and, where they are not properly treated, so fatal, that this evil is more disheartening to the new settler, than the dense forests which he has to cut down, or the venomous reptiles with which he has to contend.

During my residence in this country, in the months of September and October, 1830, I bled several individuals who resided in some of the most sickly places, but who had not yet been attacked with the fever. In every one of these, the blood invariably presented the same peculiar diseased appearance which I had often observed in those who reside near to swampy situations in the West Indies. It was very dark in colour, and evidently deranged in its physical properties, while the serum which separated, in place of being clear, had a muddy or brown colour, and, in some cases, an oily appearance. In fact, I did not meet with even one intelligent practitioner in that country who was not perfectly aware of the fact, that the blood of the inhabitants, during the sickly months, was very different from that of those individuals who arrived from healthy situations; whilst even in those who reside in the most unhealthy situations, and who had not yet had the fever, it is not only dark in colour, but evidently so much diseased in its properties, as to be very unlike the blood of health.

It is almost incredible the length of time that the narcotic poison, which causes the marsh fever, can remain in the system before it chills the blood, and paralyses the vascular organs to such a degree as to bring on the cold fit. In the climate fever, the patient is suddenly attacked, and, except that he may feel heated or uneasy, yet there is no other previous warning; but during the dormant stage of the marsh fever, there are certain premonitory symptoms which warn us of the danger, for immediately before the attack, there is a decrease of action, particularly in the vascular solids. The pulse is less frequent than in health, there is less animal heat evolved, the temperature of the blood, and, of course, of the whole body, is reduced sometimes as low as to 94°. There is generally great lassitude in the whole system, the mind is dejected, the appetite fails, the bowels are costive, and the fæces which are passed are less bilious, the tongue is foul, with a bad taste in the mouth, particularly in the morning, there is a tendency to headache, wandering pains in the bones, uneasiness in the back, the face is pale, the countenance is dejected, the eyes become heavy, the skin is sallow, and when the patient sleeps his rest is frequently disturbed with unpleasant dreams. These are the premonitory symptoms which, in this fever, precede the attack; but they are soon followed by others. During this period, there is no inflammatory or increased action in any of the solids, but the blood is diseased, not only during the premonitory symptoms, but even previous to this period. And afterwards I shall endeavour to prove that this deranged state of the vital current is not the effect of either a local disease or a nervous impression, but produced by the direct action of the poison on living blood.

The African typhus is produced by a specific *animal contagion*, and this, like the other fevers from poison, generally begins with a cold stage; but though the patients may be very cold, yet they do not shake[*] and tremble like those who are under the influence of the marsh poison. The fever which follows the animal contagion is not like the marsh remittent, a fever of type, for, even in the mildest cases, it invariably assumes the continued form. There is an expression of the countenance which is peculiar to the disease; it is not so marked as the expression in tetanus, but it is so distinct, that those who have seen it once easily recognize it again. The stomach is irritable even from the first, and the liver is affected early in the disease. The bile is exceedingly acrid; and frequently it inflames the biliary ducts to such a degree, that when the secretion ceases the surfaces adhere, and after death they are impervious even to the smallest probe. Such acrid bile must be a source of irritation to the intestinal canal, for, in some cases, it is so acrid, that out of the body it inflames the skin, and has been known to corrode the

[*] ‘ He had a fever when he was in Spain,
‘ And, when the fit was on him, I did mark
‘ How he did shake; ’tis true, this god did shake:
‘ His coward lips did from their colour fly;
‘ And that same eye, whose bend doth awe the world,
‘ Did lose its lustre; I did hear him groan:
‘ Ay, and that tongue of his, that bade the Romans
‘ Mark him, and write his speeches in their books,
‘ Alas! it cried, *Give me some drink*, Titinius,
‘ As a sick girl.——JULIUS CÆSAR.

pewter pan in which it was received, almost as fast as a strong acid. This acrid state of the bile is, probably, the cause of the severe cramps which are so often met with in the African typhus, but which so seldom occur in the marsh fever.

The poison which causes the marsh remittent may remain in the system even for months before it produces fever; whilst that which is the cause of the African typhus generally remains dormant about four days, but when applied in a less concentrated form it is often longer than this before it paralyzes the heart to such a degree as to bring on the cold stage, which, in general, precedes the continued reaction. During this period *the gums frequently become purple*, and the premonitory symptoms are similar; but, in general, they come on more suddenly, and are more marked than in those who are under the influence of the marsh poison. The blood is diseased even before the attack, and that which is drawn in the beginning is dark in colour, and decidedly in a deranged state. When first drawn, it has a peculiar smell, and coagulates *almost invariably* without any crust. There are black spots on the surface of the crassamentum; the coagulum is so soft that it can easily be separated with the fingers, and during its formation a large quantity of the black colouring matter falls to the bottom of the cup. When the serum separates, it has generally a *yellow*, and, in some cases, even a *deep orange* colour, but nothing of the bright blood-red which is so often met with in the serum of those who are in the first stage of the climate fever.

When healthy arterial blood is effused from a ruptured artery in any part of the body, where it is not exposed to the air, the serous or saline portion is removed by the lymphatics, and the denser portion that remains becomes more dark; or, even when the motion of the blood is diminished in its own vessels, the vital current suddenly undergoes changes, and the colour becomes less red. During the cold stage of every fever, as its motion diminishes the blood becomes dark; but in the hot stage it again becomes more red, for when its temperature is high and the motion rapid, the whole of its ingredients are intimately mixed, and more minutely in contact with each other. Under such circumstances, the venous blood parts more readily with its carbonic acid in the lungs, and the saline ingredients act with more force on the colouring matter; consequently, even where the blood is under the influence of a poison, which blackens its colour before the attack, it invariably becomes more red, during the hot stage, and, in some cases, it is even florid.

The following description of the diseased appearance of the blood, in the African typhus, or, as he called it, the malignant fever, is taken from Warren, who practised in Barbadoes, during the second irruption of this disease, in the year 1733. In speaking of the blood which was drawn in the hot stage, he states that it 'is florid and rarefied, with evident marks of 'colliquation, and without *the least sign of size*, that 'I could ever once observe; the insula or crassa'mentum (even when cool), upon moving it, undu-

'lates like water in a bason, and has, sometimes, 'blackish spots here and there upon its surface, *with 'much yellowish serum,* upon which it extends itself 'wide and thin *.'

Dr. Mitchell, in his History of the Yellow Fever † which occurred in Virginia, in the year 1741, says that 'in that fever, blood drawn from a vein was 'always dissolved. *The same state of the blood was 'observed in many persons who had been exposed to 'the miasmata, who discovered no other symptom 'of the disease.'* During the same epidemic, Dr. M. took away about five or six ounces of blood from the temporal artery of a patient, on the fourth day after the attack. The arterial blood was as dark as the venous; 'the serum made not above a sixth or 'eighth part of the whole, which was of a *deep 'yellow* or saffron colour, and would tinge the finger, 'or a linen rag, dipped into it, of the same colour as 'if dipped into gall,—deeper than is commonly seen 'on a rag dipped in the urine of persons in a jaundice. On this, every one that saw this blood was 'convinced that the distemper was what is generally 'called the yellow fever in America.' From this it is evident that, even at that early period, the appearance of the blood was considered the chief characteristic mark of this disease. I may also observe, that Dr. Lining was the first who noticed the fact that those who have once had this fever, have an immunity from any future attack.

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* See Warren, on the Malignant Fever in Barbadoes, &c., p. 10.

† This was evidently an epidemic of the African typhus.

I have stated that in the African typhus the blood has a peculiar smell. This is very perceptible, particularly in the last stage. The same acid odour which we perceive in the blood, is also communicated to the breath, and the degree to which this exists is, perhaps, the best criterion of the malignancy of the case. My attention was first drawn to this fact by the following circumstance.

Many years ago I remarked that a man named Benson, who had long kept an hospital in the Island of St. Thomas, for the reception of sick sailors, was, in this disease, almost invariably right in his prognostics. In some cases of this fever where the patients might have appeared to a common observer to be in little danger, for, in these there was no local pain in any of the solids, nor any increase of heat, or increase of action in the vascular organs, yet he prognosticated, from the first, that these patients would not live. In other cases, where they were suffering severely from violent excitement, he as pointedly declared that if these patients were well bled and properly treated there would be no danger. In one case, when I found him very positive, where I thought him wrong, I asked him by what criterion he judged, that he was so very certain about the result? He said, that in that fever, he only required to smell the breath, to know whether the patient was to live or die. That in cases which were really bad, although they might not appear to be so, there was always a peculiar putrid smell in the breath, and when this existed to a certain degree, he had never once known them to recover. I have since found that this is an excellent, though a very disagreeable test. I may also add, that I have since seen patients recover who had this peculiar odour to a great degree; but those cases were not treated with either leeches or gum-water, vitriolic emetics, sugar of lead, calomel, or antimony, brandy, opium, or acids.

Dr. Rush, while treating of the diseased properties of the blood in the yellow fever *, which at various periods has been so fatal in the city of Philadelphia, states that, 'to the appearances exhibited by the 'blood to the eye, I shall add a fact, communicated 'to me by a German bleeder, who followed his 'business in this city during the prevalence of the 'fever in 1793. He informed me that he could 'distinguish a yellow fever from all other states of 'fever, by a peculiar smell, which the blood emitted 'while it was flowing from a vein. From the cer-'tainty of his decision in one case which came under 'my notice, *before a suspicion had taken place of 'the fever being in the city*, I am disposed to believe 'that there is a foundation for this remark †.'

Dr. Potter, who is at present the oldest, and one of the most scientific physicians in Baltimore, observes, in his Memoir on the Yellow Fever, that 'in Sep-

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* This is the disease to which I have given the name of African typhus, from a conviction that it is originally an African disease, which has been at various periods imported into the western world from the Bulam shore, and other districts on the coast of Africa.

† See Rush, p. 223.

'tember 1800, a fact was daily obtruded on the ob'servant practitioner, which although it had long been 'familiar to me, had not then been duly estimated. 'It was remarkable that in all cases, in which it was 'deemed expedient to bleed, the blood wore the same 'general appearances. After a separation had taken 'place, the serum assumed a yellow shade, often a 'deep orange, and a portion of the red globules was 'invariably precipitated. It occurred to me, that if 'the remote cause resided in the common atmosphere, 'the blood of all who had inhaled it a certain time 'would exhibit similar phenomena. It accorded 'with the pathology I had conceived, to conclude that 'all who lived in an atmosphere so inquinated were 'constantly predisposed, and that an additional or 'exciting cause only would be required to develop 'the symptoms in form. To ascertain the appear'ances of the blood, in subjects apparently in good 'health, I drew it from five persons who had lived, 'during the whole season, in the most infected parts 'of the city, who were to every external appearance 'and inward feeling in perfect health. The appear'ances of the blood could not be distinguished from 'that of those who laboured under the most in'veterate grades of the disease. As this experi'ment might have been considered inconclusive, un'less the blood could be compared with that of those 'who lived in a purer atmosphere, remote from the 'evolution of miasmata, I selected an equal number 'of persons who dwelt on the hills in Baltimore 'county, and drew from each of them ten ounces of 'blood. The contrast in the appearances was so 'manifest, that no cause for hesitation remained. 'There was neither a preternaturally yellow serum, 'nor a red precipitate, the appearances were such 'as we find in the blood of healthy subjects. A 'young gentleman having returned from the western 'part of Pennsylvania, on the 10th of September, in 'good health, I drew a few ounces of blood from a 'vein on that day; it discovered no deviation from 'that of other healthy persons. He remained in my 'family till the 26th of the month; and on that day 'I repeated the blood-letting. The serum had 'assumed *a deep yellow hue, and a copious precipi-'tate of red globules* had fallen to the bottom of the 'receiving vessel. Of the six persons whose blood 'assumed those indications of the remote cause, four 'were seized with fever during the epidemic; the 'other two escaped any formal attack, but complained 'occasionally of head-ache, nausea, and other indica-'tions of disease. Hundreds who were not confined, 'and who never took medicine, experienced the 'effects of the general cause, under a variety of forms, 'such as nausea, giddiness, head-ache, constipation, 'a pale or yellow face, tinnitus aurium, pains in the 'extremities, and some other light shades of incipient 'indisposition. In some these were premonitory 'symptoms of a formal attack, in others they vanished, 'leaving the subject in his usual health. Many 'were listless, complaining of universal languor, in-'disposed to muscular exertion, yet did not surrender 'to the disease. They were neither sufficiently in-'disposed to be placed on the sick list, nor well 'enough to pursue their ordinary occupations.'

Dr. Potter does not appear to be at all aware of the real value of these experiments, and considers them merely as a proof of the non-contagious nature of the fever, which was so fatal in Baltimore in 1800; but this is no proof of its non-contagious character, for the truth is, that when the whole atmosphere of an infected district is saturated with a contagious poison, this will be attracted into the circulation with the air in the lungs, and affect the blood just as readily as the poison which arises from either putrid water, or from any other endemic origin. The fever, in question, was evidently, *even from the appearance of the blood*, an epidemic of the African typhus, or as they called it the yellow fever; and afterwards, I may bring forward some strong facts to prove that even in Baltimore this fever was produced by contagion. But be this as it may, the profession are greatly indebted to Dr. Potter for these interesting experiments; for if they stood alone, they prove that in this disease the blood is not merely the first, but for a time the only part of the system that feels the effect of the remote cause.

I might bring forward some strong facts on this subject from the writings of Sir John Pringle, Duhamel, Andral, &c. &c.; but these are already well known to the profession; and I trust that I have said quite enough to convince any unprejudiced practitioner, that a diseased state of the blood is in some other diseases, but particularly in fever, the original and sole cause of the morbid action which exists in the solids.

That fever is a disease of the blood, or, at least, of those fluids that are derived from the blood, is, in fact, the opinion that was first formed upon this subject, and that, too, at a period, when those who adopted it were led more by close observation and common sense, than by any idle speculations about nervous impressions, or secondary inflammation in the mucous membranes. I know, however, it may be said, that to look upon the blood as the cause of fever, is retrograding in our knowledge, and returning again to an ancient doctrine; but be it so, and if we gain by going back, the sooner we return, the better it will be, both for the sake of science, and the cause of humanity.

In the climate fever, the blood is diseased, but it is not darker in colour, before the attack; whilst in those who are under the influence of the febrile poisons, the blood is not only dark in colour, but decidedly deranged in its structure, *even previous to the commencement of the diseased action in the heart, the brain, the mucous membranes, or any of the solids.* And this unnatural darkness and derangement are so apparent, that in some instances, where the individuals have been accidentally bled, I have been able to foretell an attack of fever, merely from the diseased appearance of the blood, which had been drawn previous to the commencement of the cold stage.

We know that when the blood is dark, merely from an excess of that which is the natural cause of the venous colour, it reddens rapidly on exposure to the air, and the addition of a small quantity of any of the alkaline salts converts it almost instantly, from venous, to a colour that is both healthy and highly arterial. When the blood, on the other hand, is under the influence of any of those morbid poisons which cause fever, it remains very dark when exposed to the atmosphere: while in this state, salts may redden the colour to a certain extent; and when certain saline agents are used in excess internally, they not only brighten the colour, but counteract the effects of the morbid poison,—they add to the stimulating power of the vital current, and keep up the action of all the secreting organs, by which the greater part of the poison is probably removed. But, when these are added to poisoned blood out of the body, though they redden the colour, yet they do not produce that healthy, bright, arterial appearance, which even a less quantity of the same saline ingredients can give to venous blood, which is not under the influence of a deleterious poison, where its colour is not blackened, nor its structure injured by the morbid agent.

This dark and diseased state of the blood appears to exist previous to the commencement of all those fevers that are produced by the aërial poisons. Individuals, however, are seldom bled immediately before the attack; but accident sometimes affords us an opportunity of examining the blood, in the dormant period. During my residence in New York, in 1830, Dr. Ludlow, the author of an interesting paper on the Lake Fevers of the Genesee country, expressed a wish to see the result of certain experiments on the blood. An hour was appointed, but the individual whom he expected to bleed did not come. He went to the opposite side of the street to a young apothecary, who consented to be bled, the more readily that he did not feel very well; still he was so little ill, that he was walking about and attending to his duty. The moment that the blood was drawn, I expressed my conviction that he was under the influence of some febrile poison, and stated that the action of the salts on the colouring matter would not be so obvious in this, as in blood that was not diseased. I added a very large proportion of muriate and carbonate of soda, &c., to different portions of this blood, and though they reddened the colour, still they did not produce the same beautiful arterial appearance, which they invariably give to the blood of health. *A few hours after the bleeding, this individual was attacked with scarlet fever, and was dangerously ill for several weeks.*

While in Canada, I visited the military hospital at Quebec, with Dr. Skey, who is both an ornament and an honour to the service, and fortunately for the army, at the head of the medical department in that part of the world. Some of the soldiers had been ordered to be bled, who were in a ward where there had been a good deal of typhus fever. These men were labouring at that time under other, but not febrile disease; still it was very evident, even at first sight, that the blood, in every instance, had the peculiar dark diseased appearance, which we invariably find in those who are under the influence of a febrile poison; neither could any of the salts which I tried give a healthy arterial colour to the dark blood of any one of the soldiers, who were bled in the infected room.

It is also well known how dangerous it is to perform even the most trifling operations in an hospital, where erysipelas is prevailing, for those persons who have been confined, for a time, in such places, require only some exciting cause, to bring on an attack of that disease; and my belief is, that if the blood were properly attended to in such cases, it would be found to be diseased in almost every instance.

It is probable that in fever the greater part of the morbid poison is either changed in its properties during the disease, or thrown out of the system in its original form, by the secreting organs. In the commencement of fever the blood is dark, from the effect of the poison; but in the last stage it appears to be black, merely from the loss of its saline matter; for when we add any of the natural saline ingredients to the black fluid, which is taken from the body late in the disease, it becomes florid and more healthy in its appearance than it ever is when we mix the same, or even a larger proportion of these ingredients, with the poisoned blood which has been drawn from the system before the attack.

We have seen that there are certain poisons, both animal and vegetable, which do not produce any obvious effect, so long as they are applied merely to the sentient extremities of the nerves, even upon those surfaces that are most sensible. This is the case with the venom of the rattlesnake, and that still more deadly poison which is obtained from the wourali vine. When these poisons are applied to the surfaces of the eye, the nares, the tongue, &c., they scarcely produce any effect. They may even be taken into the stomach with the most perfect impunity; but still they are almost instantly fatal, when they come into contact with the living blood in a bleeding wound, and still more so when we inject them directly into a vein. Now this fatal result cannot be the effect of any direct impression on the nervous system,—*for these agents do not possess the power of acting on the nerves;* and probably death is produced, in such cases, by the poison destroying the vitality of the blood, and when this is effected, death must soon follow in the whole system; or if we admit that the poison ultimately makes an impression on the nerves of the internal surface of the heart and vessels, still the blood is the medium that conveys the poison, and without this the impression could neither be made on the nerves nor conveyed to the brain by these agents.

It is now, I believe, generally admitted, that all the pestilential fevers are produced by certain specific poisons which exist in the air of infected places, and whatever their origin may be, there are many facts which induce me to believe that every one of those poisons, which act as the remote causes of fever, are attracted with the atmospheric air into the circulation; mix with the blood, and produce their effect, first on the vital current, and then through the medium of this, not merely on a part, but on the whole body.

We are told in a late number of the Medico-Chirurgical Review, that 'The innumerable instances of deadly fevers being kindled up in the 'course of a few hours, after exposure to concen'trated malaria in the East and West Indies, as 'well as in several parts of Europe, prove that the 'nervous system is the first to feel the influence of the 'cause of fever*.' All this, however, is mere romance; for when such cases of instant attack are minutely inquired into, they turn out to be just as untrue as any of the tales in the fables of Æsop. During a long residence within the tropics, and though I have taken great pains to inquire into this, yet I have never known even one well-authenticated case where the fever came on, immediately after exposure to the remote cause. I have heard of people being instantly killed, who had gone into the hold of a vessel which was filled with carbonic acid, that had been generated in a hot climate from putrid potatoes; and, by some, such cases were considered as instances of sudden death from the yellow fever. Carbonic acid, however, though it may cause instant death, can no more produce the yellow fever, than it can cause the Indian cholera or canine madness. But admitting what they assert to be true; namely, that the aërial poisons produce so sudden an effect, it is no argument against the belief of their acting through the medium of the circulating current; for when we recollect the force with which the air is attracted through the medium of the pulmonary membrane into the blood, we shall not then be surprised that an active poison, which is attracted with the atmospheric air into the circulation, should derange the properties of the vital current, and produce fever in a few hours, or even in a few minutes, after it has been attracted into the circulation and mixed with the blood. But the tales of instant attack are so far from being true, that the only matter of surprise is, how these active poisons can remain so long dormant in the system, before they produce even those premonitory symptoms which precede the disease.

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* See the Medico-Chirurgical Review for October, 1829.

Those nerves which are appropriated to the organs of sense, seem to be peculiarly fitted for the reception of impressions. It is not so, however, with the nerves of the stomach; for this organ, like the heart itself, acts almost entirely independent of the brain. When in a state of disease, this, like the other vascular organs, under similar circumstances, is extremely sensible; but in its healthy state, it does not, like the tongue or the organ of smell, communicate to the brain any very vivid impression of the quality either of the food, or of those foreign agents which are so frequently taken into its cavity.

Those agents which possess the power of making a direct impression on the nervous system, are generally endowed with sensible properties; but this is not the case with any of the contagious poisons; for even those that are most virulent are so totally destitute of any sensible qualities, that there is nothing in the atmosphere of the most pestilential districts to warn us of our danger. And, however destructive these poisons may ultimately be, yet even by day, they walk as in darkness, they come unperceived, and steal upon their victims 'like a thief in the night;' for the air of the most infected district is so little changed in its apparent properties, that at the moment when the fatal poison enters the system, not one person in a thousand is at all aware that he has been breathing in any thing but a wholesome atmosphere, and at last he only finds out the danger when he is first warned by the premonitory symptoms, and soon after by an attack of the disease itself.

As a general rule, those agents, which possess the power of producing a direct impression on the nervous system, are not merely endowed with sensible qualities, but they act chiefly on the external organs of sense, and all of them produce an immediate effect. When light, for example, strikes upon the eye, the impression is immediately conveyed to the brain. The same thing occurs when a volatile agent is applied to the organ of smell, or a sapid body to the tongue. This, however, is not the case with even the most fatal of the febrile poisons, for when these are applied in their most concentrated form, they never produce fever in less than forty-eight hours. There is not, I believe, one solitary exception to this rule; and, so far as we can judge from the best-authenticated cases, even the poison of cholera never produces the disease in less than two days from the time that it is taken into the system. The poison which causes the African typhus never produces its effects previous to the fourth day, and I know some well-marked cases, where, when the poison that produces this disease was applied in a less concentrated form, the individuals were not attacked until twelve days had elapsed, after their exposure, at a distance of ninety paces, to leeward of the spot where the poison emanated.

When this disease is prevalent in Vera Cruz, strangers who arrive, on their way to Mexico, generally get away from the sickly port as early as possible, and some of them remain in the high and healthy town of Jalapa, for a short time, before they proceed on their journey into the interior. This town is high up in the mountains, about two days' journey from Vera Cruz; and though the African typhus is never known as an endemic in high and cool situations, yet there have been repeated cases of this disease at Jalapa, but always in those who had been previously in the infected commercial town on the sea-shore; and though they arrive at Jalapa in health, and remain well for a short time, yet so many cases have occurred of attacks, even so late as the eighth day, that strangers, who have been in the infected port, are never considered perfectly safe, until they have been at least ten days in the pure air. It is a fact, also, that when this disease has been raging in La Guaira, strangers who had passed through that town on their way to the high and healthy city of Caracas* have remained apparently in perfect health for several days, and then they were attacked with the same fever which was raging in La Guaira during their, perhaps, short residence in that town.

The poison of small-pox seldom, I believe, produces the disease in less than a week; and when those who have been vaccinated are exposed to this poison, if they are attacked with the varioloid, it generally makes its appearance about the same time. The following facts were communicated to me by Mr. Cormick, an intelligent surgeon in the navy of the United States of America. The cases occurred in the Pacific Ocean, on board the Franklin of 74 guns, and a complement of about nine hundred men. She had been some time in the South Sea, and the crew had been remarkably healthy, previous to the following occurrence :—

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* This large and beautiful city is built in the valley of Caracas; but still the lowest part of the valley is about three thousand feet higher than the town of La Guaira, consequently it is much cooler than on the coast. I may add, that the African typhus has raged more than once as an epidemic at Caracas, but never until after it had previously commenced in the mercantile town of La Guaira, which is, in fact, the sea-port of Caracas.

‘ It was in the harbour of Callao, and in the month
‘ of February, 1823, that I visited a seaman on board
‘ the American brig Amanda, of Baltimore, who had
‘ the small-pox. I never permitted any person to
‘ leave the boat which carried me on board, and gene-
‘ rally made the boat's crew lie on their oars at some
‘ distance, until I was ready to leave the vessel. On
‘ one occasion, a midshipman, a youth of fifteen
‘ years, neglected my instructions, and, considering
‘ himself safe by previous vaccination, was led by his
‘ curiosity to go where the man was. I insisted on
‘ his leaving the vessel immediately, which he did.
‘ This was nearly the last visit I made this man; for
‘ we sailed, very soon afterwards, for some of the other
‘ ports in Peru. *In about a week after*, this young
‘ gentleman was attacked with the varioloid*, and, in
‘ about the same time afterwards, another young gen-
‘ tleman, of nearly the same age, was also taken with
‘ the same disease. In a few days more, many of
‘ the seamen were taken sick; but the first cases we
‘ had among the men were cases of small-pox. From
‘ these we inoculated all those that we supposed had
‘ never been inoculated or vaccinated, and we found,
‘ of this description, about twenty-five. Many of those

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* It is well known that the vaccine pustule is generally fully formed in eight days; but in some cases it is much longer than this. Dr. Chapman, of Philadelphia, had a case where the pustule did not make its appearance until six weeks after vaccination. This occurred in a young lady who had been repeatedly vaccinated before, but without effect.

'we inoculated had the disease badly, but it was done hastily, and without any preparation; and, considering the difficulty we have in controlling seamen, we thought this not extraordinary. Besides the twenty-five cases whom we inoculated, there were about seventy cases more of those who had the disease spontaneously, not cases of small-pox, but of varioloid and other slight eruptions. Some of the cases of small-pox were confluent, and others of a very mild character. One man had the small-pox very badly, and he stated to me that he had had it before, having been inoculated for it, and, at the time he made the statement, I presumed he was entitled to confidence. Another seaman, a very old man, complained to me of having been unwell for some days. On examining the scalp, I found it had on it one or two decided pustules, very perfectly formed, but very small. A quarter gunner, a very large, athletic man, requested to show me his leg, where there were two very large pustules, about an inch apart and exceedingly perfect. There had not been the smallest sickness in this case whatever, and I saw a variety of cases of this description afterwards.'

The duration of the period of incubation does not always depend on the virulence of the poison. When the Dandy fever was raging in the island of St. Thomas, though this, like other contagious diseases, commenced originally in solitary cases, yet, after a time, the atmosphere of the whole town was so completely saturated with the poison, that, out of fourteen thousand inhabitants, there were not, I believe, twelve individuals who escaped the disease. Fortunately, however, it was not fatal. During this period, it was remarked that strangers, who had arrived in perfect health from a long voyage, or from healthy islands, in nine cases out of ten were attacked with this fever on the fourth day, and generally about the same hour they had landed.

The digging of that part of the Chesapeake and Ohio canal, which runs from Seneka to George-town, commenced on the 17th of October, 1828, and was nearly finished in November, 1830. The distance is about twenty-three miles, and, for the greater part, the canal runs nearly parallel and very close to the Potomac river. The bed of the canal is considerably higher, and, in some places, it is elevated fifty-four feet above the banks of the Potamac, which is there enclosed in a hilly country, and the water passes on to the ocean, with all the rapidity of a mountain torrent. During the summer of 1829, there were nearly four thousand labourers employed in making this division of the canal. They continued healthy until the end of June; but in July, August, and September they suffered severely. During these months, two out of three were attacked with the marsh fever, most of them with its most aggravated form, and, as nearly as could be ascertained, of those that were taken ill, about one-fifth fell victims to the disease.

A considerable portion of this part of the canal was cut through a bed of solid rock, and, in this place, both the sickness and the mortality were dreadful. The rain which fell into this rocky bed was retained there, and, after exposure for a short time to the hot rays of the sun, it became putrid, and the poison was evolved to such a degree, that scarcely one individual escaped the fever who was employed in that spot; but still there was not even one of the workmen attacked within *the first three days* after his arrival. It was also observed, that those remained healthy who were employed either in a gravelly or a sandy bottom, where the rain soon filtrated into the ground; whilst those who were employed in places where there was a clay bottom, soon became sickly from the marsh fever: but that part of it which passed through the rock was by far the most dangerous. These facts were stated to me, on the spot, by Mr. Fenelon and other intelligent gentlemen who were directors of the canal; and from these, as well as other circumstances of a similar kind, two inferences may be drawn: first, that water alone, when it is stagnant and becomes putrid from exposure to the hot rays of a burning sun, is capable of evolving the marsh poison, independent of either animal or vegetable matter: secondly, that the marsh poison, where it exists in the air, even in its most concentrated form, does not immediately produce fever by any direct impression on the nervous system; for those who came from healthy places, and went to work in the most sickly divisions, were seldom attacked in less than a week; and there was not one solitary instance of attack until the labourer had been, at least, *three days* in the bed of the canal.

When strangers arrive from healthy localities to visit the sickly districts in the Genesee country, during the hot months, they are seldom attacked with the marsh fever in less than a week after their arrival; and, even in those places where the whole atmosphere is saturated with the marsh poison, they are never attacked previous to the third day from the time that they commence breathing the infected air; and, in many instances, where the individuals have remained but a short time, and then removed to healthy, hilly situations, the poison has been known to remain dormant in the system for several months, particularly during the winter, and the fever only broke out in the ensuing spring, when its action was accelerated, either by heat or some other exciting cause. Such cases are frequently met with in healthy, elevated situations, where the fever and ague is not known, as an endemic disease; and, as they occur only in those individuals who have visited the flat and marshy districts the preceding autumn, there can be little doubt that the poison has remained dormant all this time in the system. In addition to this, there is another fact, which is well known to most of the physicians of swampy countries, namely, that cases occasionally occur in the middle of winter, and early in the spring, before the sun has sufficient power to cause putrefaction, or evolve the poison from stagnant water. Now, when we know that those agents which act by a direct impression on the nervous system, almost invariably produce an immediate effect, and when we know that the aërial poisons which cause fever remain dormant in the system, at least, for days, and in some cases for months, may we not infer, from this alone, that the febrile poisons, like that of the venereal, the hydrophia*, and others which act slowly, enter the circulation, contaminate the vital current, and cause disease, not by any direct impression on the brain made through the medium of the nervous system, but because they are attracted with the atmospheric air in the lungs into the circulating current, vitiate the blood, and produce their effects on the solids, entirely through the medium of their nutritive fluid.

All those agents which act on the organs of sense, probably produce their effects almost entirely on the nervous system, while most of those that enter the circulation produce their effects, like the air which we breathe and the food which we use, almost entirely through the medium of the blood. It is also, I believe, a general rule with respect to those agents which produce a direct impression on the nerves, that they not only produce an immediate effect, but when the cause is removed, the effect soon passes away. When light, for example, is withdrawn from the eye, objects are no longer visible; when a volatile agent is withdrawn from the organ of smell, or a sapid body from the organ of taste, the effect ceases almost immediately. This, however, is not the case with the narcotic poisons which cause fever, for when they are once taken into the circulation, though they do not produce any immediate sensible effect, yet the result will be sure to follow, even though the individual be immediately removed to a healthy situation, where there is not one particle of the poison except what he has in his own system. This occurs, also, with the poison which is the cause of canine madness, the venereal, and other poisons, which enter the circulation, and act upon the general system through the medium of the blood; but I know of no instance where an agent that acts by a direct impression on the nervous system produces no effect whatever, when it is first applied, and where the effect comes on, days, weeks, or perhaps even months, after the individual has been far removed from the original cause which had produced the impression on the nervous system. Now, for my own part, I believe it as impossible for light to produce no effect whatever, when it is first applied to the eye, and then that objects should long afterwards become visible in the midst of darkness, as believe that those agents act by a direct impression on the nervous system, which produce no effect whatever when they are first applied, and then produce disease long after the individual has been far removed from the original cause of the sickness. And when we know that these poisons are totally destitute of any sensible qualities; when we see that they do not produce any immediate effect, either on the nerves or the brain, but remain dormant in the body, for days, weeks, and, in some cases, even for months; when we see, also, that the effect follows, when the infected individual is far removed from the original cause; but, above all, when we see, that from the period that the poison is taken into the system up to the moment of attack, the blood is not only the first, but, for a time, the only part of the whole system that feels the effect of the remote cause, we are then led to infer that fever is not the effect of a nervous impression, or of inflammation in a part, but, in the first instance, a disease of the blood, and then, when this is affected, the nerves, the brain, and the whole of the solids suffer, as the inevitable result of the diseased state of their nutritive fluid; for when this is under the influence of a febrile poison, the whole system must soon be involved in universal disease.

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* Dr. Gregory considers the average period of incubation of the hydrophia germ at forty-five days: the minimum twenty-one days, and the maximum nine months.—See No. II. Cholera Gazette.

During the dormant period which precedes the attack, the poison itself either undergoes certain changes, and its narcotic properties become more active, or it chills and produces certain changes in the blood, which renders it almost incapable of stimulating its own vascular organs, and then comes on that cold stage which is so invariably met with in all those fevers which are produced by the aërial poisons.

Wherever an agent produces an impression, there the effect, in general, is first felt. An excess of light, for example, can only injure the eye, an intensity of sound can only affect the ear, and cow-itch produces irritation only in that part of the skin to which it is applied. Those noxious agents that produce their effects by a direct impression on the nervous system, act with force on the brain; but those that act through the medium of the blood produce their first effect, not on the brain, but on the vascular organs. The heart is, by far, the most irritable organ in the whole body; and of all the solids, this is the first to live, and the last to die. It is the first to act in the fœtus, and, in many instances, it retains its vitality even long after the brain has completely ceased to perform its functions; while, in some cold-blooded creatures, the heart will continue to contract and re-act on the application of stimuli, twenty-four hours after the head of the animal has been cut off. The blood is the natural stimulus of the heart; and the internal surface of this organ is, perhaps, more keenly alive to the quality of its stimulus than any other surface in the whole system. When we distend its cavities with air or water, in place of blood, it instantly ceases to act, but re-acts with force when we inject a *saline* fluid into the blood. Now, in fever, the black and poisoned blood evidently produces its first effect, not on the brain, but on the heart and the other vascular organs, and these, for a time, are chilled into a state of complete torpor. There is then a want of circulation in the whole body, but particularly in the extreme vessels, in consequence of which there is, for a time, a cessation of that process by which the animal heat is evolved. But in such cases re-action is the road by which the animal economy marches to health; for, as we have seen, the living principle is endowed, to a certain degree, with the power of resisting the effects of those noxious agents that are destructive to life. When the poison is very virulent, as in the African typhus, or where the individuals are weak, the patients sometimes die during this cold stage; but if they are properly treated, re-action, in general, soon comes on: still, even in this stage, the blood and its organs are the primary and chief seat of the disease. The re-action commences first in the extreme vessels, an increase of animal heat is evolved, and the temperature of the venous blood soon becomes higher than 98°, the cause of its impurity is rapidly removed in the pulmonary organs; but besides this, the increase of heat in the blood adds greatly to its stimulating power. The heart now acts with force; there is soon, in most fevers, violent excitement in the whole vascular system, and during this struggle the deleterious agent either causes death, or the poison itself is decomposed in the body, or, perhaps, it is thrown out of the system by the secreting organs, in its original form.

The circulating current is endowed with a vital principle, and healthy arterial blood contains also a portion of pure air, which it receives in the lungs, and this is diffused in the other material ingredients which enter the circulation through the medium of the thoracic duct. It is by these grosser ingredients, air and vitality, that the blood is formed; but the vital principle, which exists in the blood, is as easily affected by noxious agents as either the brain, or any of the solids.

We have seen, that the first effect of the oxygen in the pulmonary organs is, instantly to remove the cause of the impurity from the venous blood; we have seen, also, when this is effected, the force with which the atmospheric air is attracted into the circulation: now, we can readily conceive that, in an infected district, or in a confined room where the air which we breathe is saturated with a poison, this subtile fluid may be attracted into the circulation along with the air, mix with the blood, derange its properties, and affect its vital principle, without producing any direct impression on the nervous system. When it enters the circulation, the poison remains dormant for days; but we have no proof that it is concealed either in the brain or the nerves. We have seen, however, that it deranges the blood and blackens its colour, previously to any affection of the solids, or before it either produces the cold stage or causes reaction. The poison itself is the remote, but the vitiated state of the blood, produced by the poison, is the immediate cause of fever. And that such is the fact, is, I believe, just as certain as that narcotic poisons, when injected into a vein, can instantly destroy the vitality of the blood, and cause death without producing or leaving the slightest trace of disease in any of the solids.

There are other agents, as well as the contagious poisons, that do not cause fever, except when they mix with the blood. When putrid water, for example, which contains the germs of the marsh poison, is merely applied to a nervous surface, it does not produce sickness; yet it is well known, that when a very small portion of a putrid fluid is injected into a vein, it produces fever with symptoms much more malignant than those that are generally met with in the milder fevers of cold countries.

There are certain poisons which produce no evil effect when first taken into the stomach of inferior animals; but we have positive evidence that they enter the circulation, and after a time they appear to be thrown out by the secretions in so concentrated a form as to produce fever, and sometimes even death, in those that use the milk which is derived from the poisoned blood of these animals.

This is remarkably the case with a small indigenous plant which grows in Tennesee, on the banks of the river Cumberland, and in other parts of the western district of the United States of America. This plant is called by the natives the 'Indian Hachy:' it has blossoms of a blueish colour, and is so very poisonous, that the milk of the cows that have fed upon this plant is sometimes so saturated with the poison, that the small quantity used with tea produces fever, with the most violent symptoms, and, in some cases, even death.

Dr. M'Call, who has written an account of this poison in the Philadelphia Journal, for 1822, says, that 'after swallowing the milk, the person in a 'short time suffers from thirst, nausea, vertigo, con-'fused or imperfect vision, vomiting often ensuing, 'succeeded by a violent fever, the exacerbations sub-'siding at irregular intervals. The pulse is extremely 'variable, sometimes strong and full, at others tremu-'lous, small, and corded. Constipation, which exists 'from the beginning, becomes more obstinate towards 'the third or fourth day. The skin, also, about this 'time, is more hot and parched, the eyes are red and 'suffused, there is very great restlessness, and all the 'secretions are scanty. Towards the sixth or seventh 'day, excessive debility takes place, with, very often, 'paralysis of the tongue and other parts, and soon 'after ensue stupor, cold clammy sweats, convulsive 'hiccough, and often offensive cadaverous odour, and 'death.'

Dr. Haines, who has also written an interesting account of the disease produced by this poison, which, by the natives, is called the 'Trembles,' observes, that 'the heart beats with such violence, in some cases, 'as to excite horror in the physician and bystanders. 'When they lay the hand upon the patient's breast, 'it seems to labour convulsively, and as though it 'were clogged in its action by a superabundance of 'blood; the patient feels nothing he can strictly call 'pain, but the sense of heat, the oppression, the palpitation of the heart, and the violent efforts to vomit, 'constitute an extreme degree of distress.

'That the milk, the flesh, &c. of any animals, 'killed while labouring under this disease, will produce disease again in other animals, is proved by 'daily experience. Sucking calves, which had no 'food but the milk of an affected cow, will show the 'peculiar symptoms, and often die of the disease. 'Persons making use of the milk or butter from the 'same cow, at the same time, will become affected.

'I saw an instance of a whole family becoming 'sick with this disease, some of them in a few hours 'after dining upon a loin of veal, in which it was 'afterwards satisfactorily ascertained, that the calf 'laboured under the disease at the time it was butchered, being sold in the market by an unprincipled 'person. I well recollect, also, an instance of several persons becoming severely affected, from incautiously eating of a pig which had been fattened 'upon the milk of a cow that was known to be affected 'with this disease; all of the family who ate of it 'were seized, some of them in a few hours: there 'were circumstances attending this case, admitting 'no doubt of the source from which it originated.'

Now, in this case the poison is taken into the stomach of the inferior animals, without producing any apparent impression on the nerves of the stomach. When it enters the circulation, it is rapidly removed by the secreting organs, but in so concentrated a form, that the milk produces the most fatal effects, not only in the human subject, but in inferior animals, who either drink the milk or eat the flesh. When a human being drinks even a small quantity of the poisoned milk, it does not appear to produce any direct impression on the nerves of the stomach; and, from the time that it requires to act, it is probable that it is immediately absorbed or attracted into the blood; and the burning in the gastric organs begins only a few hours after, when an attempt is made to throw it out by the various secretions; but the fever, and the burning at the stomach, &c. &c., are merely in this, as in other fevers, the effects of the poisoned or diseased state of the whole circulating current.

The then editor of the Philadelphia Journal was a great advocate for sympathy or nervous impressions; but as this poison evidently acts through the medium of the blood, he was very willing to attribute this disease, not to the vegetable poison, but to that universal source of all evil, marsh miasmata; but as miasma does not produce the same disease, or similar symptoms in any other part of the world, he gets rid of this difficulty, by calling it *a modified miasma;* but it must be rather a curious sort of modification that makes it produce its effects in so peculiar a way, only in those districts where this plant grows, and only in those unfortunate individuals who have used this specific poison internally, a short time before the attack, and to spare others, who are breathing the same air, but who take care not to taste the poisonous food.

Magendie confined a healthy dog in a situation where he was exposed so as to breathe the exhalations from substances in a state of putrefaction. These produced no effect during *the first four days;* but he then became emaciated, and died on the sixth. This certainly does not look like a case of death from either local inflammation, or an agent that acts by a direct impression on the nervous system.

There is one poison that has been very fatal in some parts of Germany, which produces no effect whatever on the solids for nearly twenty-four hours after it is taken into the stomach. Now we can scarcely believe that any poison, which produces its effects by a direct impression on the nervous system, should remain dormant and inactive in the stomach for twenty-four hours; while from the symptoms produced by the poison, and the length of time that it takes to act, there is every reason to believe that it produces no effect whatever on the nerves of the stomach until long after it has left the gastric organs, entered the circulation; and even then, the diseased action in the solids is only excited when the secretions in the stomach and the other organs are deranged from being supplied with blood that has been vitiated by the poison.

Innumerable examples might be brought forward to prove that disease and even death may be produced by a deranged state of the blood. Vernière introduced nux vomica into a wound in an animal; a part of the poisoned blood was then taken from a vein which led from the wound, and injected into the vein of another animal. The animal into which the poisoned blood was injected died with locked-jaw, the same as if the poison had been originally applied to a wound in some part of its own body. It was also proved that when the diseased blood was withdrawn, or prevented, by a ligature, from entering the circulation, the effect on the animal was completely prevented.

If the production of the same disease, by the injecting of poisoned blood from a diseased animal into the veins of one that is healthy, be a proof that the poison acts through the medium of the circulating current, we have sometimes evidence equally certain in the human body, that the poisons which act as the cause of the essential fevers, produce their effects entirely through the medium of the vital fluid, for the unborn child has neither organs sufficient, nor materiel within itself for the formation of its own blood; its whole supply is, therefore, evidently derived, either directly or indirectly, from the blood of the mother. We have reason to believe that there are no nerves in the umbilical chord; and we know also that the nervous system of the child, before birth, is in a state of complete torpor; yet the diseased blood of the mother can communicate to the fœtus in utero the poison of small-pox, &c., and this poisoned blood causes the same specific fever in the child, and that, too, at a time when the virus could only have been conveyed through the medium of the blood, for the poison, in this instance, could not have been applied in any way, so as to produce the disease by a direct impression on the nervous system.

Lind, I believe, is one of the first authors who notices intermittents as appearing among infants at the breast. The following cases are related in Dr. Ludlow's Observations on the Lake Fevers in the Genesee country.

'Mrs. W., wife of Dr. W., was delivered of a 'child during a course of the fever and ague. 'Within *three weeks* after birth, the child was attacked with the same disease, and underwent a 'long course of it.'

'Mrs. G. was attacked with fever and ague about 'the first of August. The disease continued, with 'some interruptions, during the autumn, winter, and 'spring. In May, about a week after the paroxysms 'had ceased, she gave birth to a daughter, who, 'within eight or ten days, was attacked with the 'same disease, and which continued, with some interruptions, for nearly two years, during which time 'little hope was entertained that the child would 'survive. It finally recovered; but was always 'weakly. The mother never afterwards had the 'disease.'

'Mrs. R. had fever and ague at two different 'times. During the last stage of her last pregnancy 'intermittents were very prevalent. Having for 'several days suffered some of the precursory symp'toms, she was, on Sunday afternoon, attacked with 'a severe paroxysm. Every stage of it was regular 'and distinct, the paroxysm terminating in diaphoresis. 'On Monday morning she was delivered of a boy, 'apparently at the full time. On *Monday afternoon*, 'at about the *same time of day* at which the Sun'day's paroxysms had occurred, the *child was at'tacked*; the cold stage was severe and long; the 'skin being livid, and the child was thought to be 'dying. This was followed by the hot stage, and, 'in due time, by diaphoresis *. The paroxysms con'tinued to recur daily for about a fortnight, when 'small doses of Peruvian bark were given. The 'disease soon ceased; but in about a week the child 'had two fits more, when the bark again arrested it. 'The child is now more than two years old, is fat 'and healthy, and has had no more attacks of the 'fever. The disease *did not recur in the mother 'after delivery*. Both still reside in the same house, 'which is situated on aguish ground.'

'These cases seem to prove very satisfactorily, 'that the disease is sometimes congenital. The

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* In this case it is very evident that the poison had been communicated from the mother to the child, through the medium of the maternal blood; for when this poison is taken in by the lungs, it never produces fever in less than three days; yet this child was attacked in a few hours after it had drawn its first breath.

'second case appears to show that a regular course 'of disease in the mother, during pregnancy, does 'not remove the susceptiblity of the fœtus in utero. 'The third is remarkable for the regular transfer of 'the disease from the mother (in whom the susceptibility had been worn out) to the child, who, like 'others who had never had the disease, was perfectly 'susceptible of it.'

It is well known that the venereal disease may be communicated to the fœtus in utero. It is also a fact that a healthy child, after birth, may become affected with the same disorder, by using the diseased milk of an infected nurse. Two remarkable cases, in confirmation of this fact, occurred in the practice of Dr. Hosack, of New York, in the year 1807. 'A healthy 'infant, born of respectable parents, was placed under 'the care of a wet nurse, and in about four weeks 'after, eruptions appeared in different parts of its 'body. The ordinary alteratives were had recourse 'to without effect: the child became worse; ulcers 'on the throat, and other symptoms, strongly resembling those of lues venerea, were observed. Mercury was now administered in the form of the solutio 'oxymur. hydrargy. by which the disease was completely removed. Suspicions being thus confirmed 'as to the nature of the disorder, the infected nurse 'was dismissed, with an injunction to undergo a 'mercurial course. Shortly after, and regardless of 'advice, she entered into another family, in which 'she again communicated *to a healthy sucking infant* 'the venereal disease, which yielded only to the operation of mercury. These two cases render it no longer problematical, but unequivocally prove that an infected nurse may, merely by her milk, communicate a specific disease to her nursling. They also still further render valid the opinion that not only the blood, but the secretions may, to a certain degree, be assimilated in their nature to the virus of certain morbific poisons *.'

'Poisons,' says Mr. Hunter, 'take their different seats in the body, as if they were allotted to them.' The poisons which produce the small-pox, the measles, &c., enter the circulation, mix with and become part of the blood. They first paralyze the heart; in a short time, however, reaction or fever comes on, and, after a struggle, they are thrown out, partly by the secreting organs and partly by the skin. The poison which produces the African typhus is most destructive to the biliary organs; that which produces the dandy fever acts chiefly on the smaller joints; the poison of hydrophobia fixes chiefly in the throat; that taint in the system which causes scrofula, fixes almost entirely on the lymphatic glands in children; but at a more advanced period of life it attacks the pulmonary organs, and causes a slow but certain death; while the venereal poison attacks the skin, the nose, the throat, and the periosteum of the bones.

The aërial poisons which cause fever are not

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* See Dr. Francis on Mercury, in the American Medical and Philosophical Register, p. 488, vol. iv.

mere varieties of the same agent; for each of the specific poisons produces its own specific disease. The poison, for instance, which causes the marsh fever, can no more produce small-pox, the African typhus, the plague, or cholera, than the seed of an apple can produce the orange, or the seed of the orange can produce an oak. When some of these poisons enter the system they appear to possess the power of multiplying themselves. One grain, for example, of the poison which produces the small-pox, applied to a wound in the arm, produces, in a short period, many ounces of the same specific virus. This may be the effect of secretion; but the secreting vessels can only secrete the same specific matter from blood that is tainted with the same specific poison. When the poison of small-pox is taken from a pustule in the foot, it possesses the identical properties with that which had been originally applied to the arm. Now it is very evident that this poison could not be carried from the arm to the foot by sympathy, or through the medium of the nervous system; for this diffusion of the virus can only be effected through the circulating current: and the same vitiated state of the blood which produces the morbid action in the solids, is also the cause of the diseased condition of the secreted fluids; for, as not one drop of pure water can issue from a poisoned fountain, neither can the secreting organs give out healthy secretions from blood that is so much vitiated, as it is in the various forms of pestilential fevers.

Fever is sometimes met with in parts in which neither the brain, the nerves, the stomach, nor the mesenteric glands can have any direct share in producing the disease. I have seen fever or febrile action in the paralyzed lower extremities of a person who had long been afflicted with a diseased spine. The nerves had long ceased to have any influence over these extremities, yet when the person was exposed to miasma, and remitting fever came on, the lower extremities became colder during the cold stage, and in the hot, the excitement was nearly as strong in the lower extremities as in those parts of the body, where there was a direct communication with the brain. Now as the nervous power in these limbs was entirely lost, the fever in them could only be produced and kept up by the vitiated state of the circulating blood.

Constitutional sores act often as drains, established by the preservative power, to throw some vice out of the body. When these are incautiously or suddenly suppressed, the vice is retained in the circulation, and fever soon follows the suppression of the discharge.

In certain inflammations of a specific character, the diseased state of the blood is, probably, the sole cause of the morbid action which occurs in the solids. This is the case in erysipelas, in the secondary symptoms produced by the venereal poison, in erythema mercuriale, gout, rheumatism, carbuncle, peripneumonia notha, &c. &c. Common inflammation, however, is generally the effect of local injury done to the solids, local irritation, &c. &c.; but even in common inflammation the whole circulating current becomes deranged, as is evident from the firmness with which the fibrin contracts, the cupped appearance of the clot, and the white albuminous * crust which almost invariably forms on the surface of the crassamentum. This diseased or inflammatory state of the blood is, probably, the effect of the local disease; but it is equally probable that this is the cause of the symptomatic fever which is almost sure to follow inflammation when the blood is deranged to a certain extent.

The mind has an influence over the motion of the blood, and there are many reasons for believing that the nervous, like the electric fluid, can produce a sudden change in the quality of the whole circulating current. Excessive grief, violent pain, &c., may derange its physical properties, and this derangement of the blood is, probably, the immediate cause of the fever which sometimes follows; for often, when such cases are fatal, no appearance can be found in the solids to enable us to explain the cause of death; while the blood is invariably found to be dark in

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* I prefer calling this the albuminous crust, from a belief that it is formed not by the fibrin, but by the albumen of the blood, which has no affinity for the colouring matter; and in certain diseased states of the blood, this coagulates on the surface so as to form a white crust, without any intermixture with the hæmatosine, except occasionally a few particles that are held in a state of mechanical suspension.

colour, in a fluid state, with little disposition to coagulate, either while in the vessels or when exposed to the air.

Jaundice, in some cases, is produced by a moral cause, and comes on at a time when there is no pain, nor any disease or derangement in the liver, to account for either the derangement in the blood or the change of colour in the skin; while coma and other symptoms occasionally occur when there is no affection of the solid structure of the brain to produce these phenomena. After death, the membranes of the brain, and the serous fluid which is found in the ventricles, have a deep yellow colour, and this colour is produced, as in those who die from the African typhus or from the bite of a venomous snake, not by an excess of bile, but by the vitiated state of the vital current.

It is well known, that the colour changes both in the blood and the skin, in those who have been bitten by venomous reptiles; and, in some instances, where blisters have been applied to individuals who have been under the influence of the poison of the rattlesnake, the serous fluid which exuded had a green colour *.

Mosely states, that 'In the expedition from Jamaica, in 1780, on the Spanish Main, a soldier of 'the 79th regiment, in marching through the woods

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* I may also observe, that I have seen greenish streaks on the crassamentum of blood that had been drawn from lepers.

'near *San Juan* castle, was bitten by a serpent 'hanging from the bough of a tree, under the orbit 'of his left eye, from which he instantly felt so 'much pain that he was unable to proceed. He died 'in a few hours, with his body considerably swelled, 'and of *a deep yellow colour*. The eye, near the 'bite, was entirely dissolved.'

Ulloa says, 'That a person bitten by the *coral* '*snake* immediately swells to such a degree, that 'the blood gushes out through all the organs of 'sense, and even the coats of the veins at the extre'mities of the fingers burst, so that he soon ex'pires.'

Now that the citric acid is fortunately no longer in general use, as a preventive against scurvy, this disease has become, comparatively speaking, extremely rare; and though salted provisions be used as much as formerly on board of the vessels, still I have never seen even one case of scurvy amongst the sailors during the twenty years that I resided in the West Indies; consequently, I have had no opportunity of examining the vitiated state of the blood in this disease. Some, however, of the older writers have left us interesting descriptions of the appearance which is met with in the blood of those who are afflicted with this complaint. Dr. Mead states, that 'In the beginning, as it flowed out of the orifice of 'the wound, it might be seen to run in different 'shades of light and dark streaks. When the 'malady was increased, it ran thin, and, seemingly, 'very black; and after standing some time in the 'porringer, turned thick, of a dark muddy colour, 'the surface in many places of a greenish hue, 'without any regular separation of its parts. In the 'third degree of the disease, it came out as black as 'ink; and though kept stirring in the vessel many 'hours, its fibrous parts had only the appearance of 'wool or hair floating in a muddy substance.

'In dissected bodies, the blood in the veins was 'so entirely broken, that by cutting any considerable 'branch you might empty the part to which it 'belonged of its black and yellow liquor. When 'found extravasated, it was of the same kind. And 'lastly, as all other kinds of hemorrhage are frequent 'at the latter end of the calamity, the fluid had the 'same appearance as to colour and consistence, 'whether it was discharged from the mouth, nose, 'stomach, intestines, or any other part.'

When acids are used internally some of them enter the circulation, darken the colour of the current, and reduce the force of the vascular organs; or when we add a weak acid solution to healthy blood out of the body, we make it quite as black as it is either in scurvy, cholera, or the the last stage of the climate fever; consequently, such remedies ought never to be used in malignant diseases, where the blood is already in this black and vitiated condition. These, however, have been extensively used from a belief that they contain oxygen in a separable form, which would redden the colour of the black blood. I need scarcely say, that under this treatment the mortality was dreadful, particularly in the malignant fevers of hot countries: and with respect to scurvy, it would not be difficult to prove, from the writings of Trotter and others, that this disease, which at one period was so distressing in the British navy, was frequent and fatal almost exactly in proportion to the quantity of citric acid, which was used as a preventive, and, unfortunately also, *as a cure* for the disease.

In the blood of those who are labouring under chlorosis, there is a large proportion of water, and a great diminution of all the solid ingredients, but particularly of the hæmatosine; consequently, in this disease, the blood has less consistence and less redness than it has in health. In some instances it is so pale, that it scarcely gives a red tinge to white paper or linen; whilst, in others, it is so unlike healthy blood, that when drawn from the body, it generally resembles a mixture of water and dark-coloured beer. Now, when the blood is in this inanimate, thin, and diseased state, we need not be surprised at its want of power in stimulating the heart, or that there should be a want of action in the extreme vessels, and less heat evolved in the whole body. There is also a diminution of secretion in the various organs, with loss of appetite and dislike to exercise. The skin becomes deadly pale, and, in some cases, has a yellow tinge, whilst in others it changes to a colour that is almost green. The lips lose their redness, the eyes are encircled with a livid areola, and the whole body has a leucophlegmatic appearance, accompanied with palpitation at the heart, difficulty of respiration, fœtid breath, acidity in the stomach, and a morbid appetite for alkaline agents. When the alkalies, or alkaline carbonates, are used in such cases, they not only relieve, as in fever, the burning at the stomach, by neutralizing the acid, but they form a salt in the gastric organs, which enters the circulation, reddens the colour of the blood, and adds to its power of stimulating the heart and the various organs.

In one specimen of diseased blood, 'which was 'taken from a young woman of a most sickly chloretic 'appearance,' and examined by Dr. Hodgkin and Mr. Lister, this blood had a thin coat on its surface, not like the usual buffy coat, but of a dingy brown, somewhat resembling cream intermixed with coffee, whilst the crassamentum was of a dark and dingy colour. The particles of this blood, as seen by the aid of Mr. J. J. Lister's very powerful microscope, were very little altered in form, but, perhaps, not quite so well defined as the particles of the blood generally are.

It is now well known that the blood is decidedly diseased, even in the common typhus of more northern climates. Dr. Tweedie, who has had ample opportunities of forming a correct opinion on this subject, states, 'that in this class of fevers the 'crassamentum of the blood, instead of forming a 'firm coagulum, is loose, small in proportion to the 'quantity of serum, and so soft that it breaks readily 'on attempting to raise it, resembling in consistence 'half-boiled currant jelly; and that, in some in'stances, when abstracted late in the disease, it is 'scarcely coagulated at all.' Now, when we recollect how impossible it is for the heart, or any of the organs, to perform their healthy functions without a regular supply of healthy blood; and when we see that, even in typhus, the whole of the vital current is so much diseased, can anything but the blighting effects of preconceived opinions, obstinately retained, because founded on prejudices which have taken a deep root, induce us to believe, even for a moment, that fever ought to be treated, as it generally is, merely with a reference to the increase or decrease of action in the solids, and without any attempt on our part to remedy this vitiated condition of the whole circulating current. Now, is it not evident, that when this, the stimulating and nutritive fluid of all the solids is so much diseased, there must be universal derangement in the whole system; for, under such circumstances, every one of the secretions must be diseased, and not one of the organs can properly perform its healthy functions, when supplied with such blood as we invariably meet with, not merely in one, but in all the essential fevers that are met with, not only within the tropics, but in every part of the world.

In all the eruptive fevers, the whole of the phenomena are evidently not produced by any nervous impression, but more probably by the vitiated state of the blood, and most of the symptoms are apparently caused by an effort of the preservative power to throw the poison out of the circulation. When the poison of small-pox is applied to a puncture in the arm, it does not produce any immediate or direct impression on the nervous system, or any local inflammation sufficient to cause fever. The puncture which is made by the poisoned lancet, is not more painful than a similar incision made with one that is perfectly clean, and in a very few minutes the pain is scarcely felt. Several days elapse, and no effect is apparently produced, until the poison be absorbed, and become part of the vital current. After a time this poisoned blood paralyses the heart; reaction, however, soon comes on, and the fever continues until the poison be thrown out, partly by the skin, and partly, perhaps, by the secreting organs: when this is effected, then, and then only, the fever abates.

In the fevers that are produced by the animal aërial poisons, there is generally but one continued struggle from beginning to end; but in the marsh remittent, paroxysm generally succeeds paroxysm with alternate rest, until the poison be thrown out of the system, or until it be neutralized by the use of proper remedies. At the end of each paroxysm the quantity of fluid that is thrown out lessens the volume of the blood, while the cold produced by its evaporation lessens the temperature of the whole body. It is not spasm in the extreme vessels, but, as I believe, the presence of the specific poison in the circulation that causes the return of the paroxysm; for it often returns almost immediately after a profuse perspiration. But why the animal poisons should always produce a continued fever, as in the African typhus, the small-pox, &c.; and why the marsh, or that poison which arises from putrid water, invariably produces a fever of type, with alternate paroxysms, as in the marsh remittent, we know no more than we do why the animal poisons should be able to propagate themselves, while that of the marsh fever becomes totally inert after it has passed through the living body. We know the fact, and this, at present, is the amount of our knowledge on this subject.

When fevers are produced by a poison in the system, each specific poison produces its own specific disease; yet, in general, they have some symptoms that are common to them all; but in fever, a similarity in certain symptoms does not establish a sameness in kind. All the contagious and miasmatic fevers are generally preceded by uneasy feelings. The remote causes which produce these fevers first poison and chill the blood, after a time they paralyse the heart, and this paralysis in the vascular organs is the immediate cause of the cold stage; they generally produce irritation in the stomach, and often diseased action in the liver, the spleen, &c. In these fevers, the gastric juice, the bile, and all the secretions are evidently deranged, and probably contain a part of the poison, which is thrown out of the circulation by these channels. In all the fevers from poison, irritation of the stomach is so common, that those physicians who have only had an opportunity of seeing such fevers, were led long ago to believe that irritation of that organ was the sole cause of the disease. Baglivi, who practised near to the swampy districts of Rome, adopted this belief long before M. Broussais; but had either of these physicians practised in some of the hot and dry mercantile towns in the West Indies, they would soon have been under the necessity of looking for another cause: for, as I have said, this symptom never exists in the beginning of the climate-fever, unless when the patient has committed a debauch immediately before the attack, and, in that case, the irritability of the stomach is the effect of the intemperance, and not of the fever.

It is but a poor objection to say, that neither the contagious poisons nor the marsh miasma can be detected in the blood by any chemical test. Those agents, like the vital principle, or caloric, are invisible; but, like vitality, or the cause of heat, the aërial poisons produce the most visible effects. Whatever the origin of these agents may be, it is now, I believe, generally admitted that poisons often exist in the atmosphere, acting as the remote cause of fever: and if chemists do not yet possess any test to enable us to detect them in so simple a fluid as atmospheric air, we can scarcely expect to find them in one that is so complicated as the blood. When chemistry can detect them in the one, the same test may enable us to prove their existence in the other; until then, we may believe that they exist in the blood, not only from the visible effects they produce in that fluid, but from the same evidence that we believe in their atmospheric existence, that is, from their effects; for, as yet, we have nothing else to enable us to prove that these poisons ever exist as the remote cause of those fevers which we believe to be produced by the aërial poisons. But when the air produces, in those that breathe it, a specific fever, with a cold stage, an irritable stomach, a foul tongue, derangement in the biliary organs, diseased secretions, and the other symptoms of contagious or miasmatic fevers, we then believe that such air contains a poison. For the same reason, when the poison enters the system unperceived, and without producing any immediate effect on the nervous system—when it remains dormant for days in the body, without producing any change, except in the blood—when we see that the whole current is dark in colour, and diseased in its appearance, even before the attack, when this diseased blood first paralyzes the heart, and then produces fever, with an irritable stomach, a foul tongue, and the other specific symptoms peculiar to this class of fevers, we may then, on the same evidence, believe that the poison has entered the circulation, and that this is the cause; while the paralysis, the reaction, and the other symptoms which occur in the solids, are merely the effects of the diseased state of their nutritive fluid.

The blood is the natural stimulus of the heart; and the healthy action of this organ depends fully as much on the quality as it does on the quantity of that stimulus which causes it to contract. When we inject a small quantity of tartarized antimony into a vein, it mixes with and deranges the whole current; the blood then possesses new and diseased properties. This vitiated or poisoned blood first paralyzes the heart, and during this state of paralysis in the vascular organs, we have symptoms, for a time, similar to those which exist in the cold stage of those fevers that are produced by the aërial poisons. An effort, however, is soon made by the preservative power, to throw the foreign agent out of the circulation by the stomach, the skin, &c. &c. The portion which is thrown into the stomach produces great irritation in that organ; the peristaltic motion is reversed, and vomiting is produced, which continues until the greater part of the foreign agent is expelled from the circulation. Both the contagious and the marsh poisons have a similar emetic effect, and by deranging their secretions they produce great irritation, tenderness, and, in some cases, even pain, in the gastric an biliary organs; and the frequency of these symptoms, both in dense populations and swampy districts, led to the belief, that the liver and stomach were 'the very seat 'and throne of the disease!' These symptoms, however, do not exist in that which is the most ardent of all the fevers of the Western world; and where they do exist in those fevers that are produced by the aërial poisons, the deranged state of the liver, and the irritation of the stomach, are as much the effect of the vitiated state of the blood, as they are when we derange its properties by injecting tartarized antimony or putrid water into a vein.

There is no complaint so common as fever; yet this is the disease of all others the most difficult to define. Its phenomena are exceedingly complex; and, in many instances, those symptoms are entirely absent which appear in others to be the most essential to the disease. Boerhaave struck from the list of symptoms all those which he considered as not essential to fever; and out of the whole he retained only three, namely, a chill in the commencement, followed by increase of action and increase of heat; but he might, with equal propriety, have left out these, as well as the others, for in the climate or seasoning fever of the West Indies, there is seldom or never any chill, in the first stage; and in some of the most malignant cases of the African typhus, the marsh fever, cholera, &c., the patients die during the chill; consequently, in those cases that are most fatal, the cold stage is not followed either with increase of action or increase of heat. But, independently of this, I have seen some malignant cases of the African typhus, where there was scarcely any chill in the beginning, and so far from any subsequent excitement, there was a want of action from the period of attack up to the moment that the disease terminated in death: these symptoms, therefore, are not essential. Neither can we consider fever, at least such as we observe it within the tropics, as the result of either a nervous impression or local inflammation; for, even in the beginning of fever, its symptoms are universal and peculiar to itself. It is not, therefore, a local affection; and in all the idiopathic fevers, but particularly in those that are produced by the aërial poisons, there is but one thing that is never absent, namely, the diseased condition of the whole circulating current, and, therefore, this alone can be fairly considered as essential to the disease. This morbid condition of the blood is, decidedly, the first link in the chain of those phenomena which constitute fever, for, even before the attack, every drop of the vital current is changed in its properties, and wherever this deranged blood can circulate, there fever extends its empire: for the cause which produces this disease is not confined to a part, but acts on every fibre and in every tissue of the living system; it disturbs every function in the body, and deranges every faculty of the mind. All the excretions are in a diseased state, and every one of the secreted fluids is changed both in its quantity and quality. The blood, however, is the first part of the system that feels the effect of the remote cause. It is probable that this poisoned or vitiated blood may ultimately produce an impression on the internal irritable surfaces of the heart and vessels, and the brain may be affected either directly by this impression, or indirectly by the diseased quality of the blood which is sent to that organ. But, be this as it may, still the blood is the medium that conveys the poison, while the impression on the nerves is merely the effect of the diseased condition of its natural stimulus. When the chill commences, the heart, for a time, acts feebly, but the cold stage is generally soon followed by reaction, and during this, the remote cause is either rendered inert in the system, or an attempt is made by the preservative power to remove the deleterious agent from the circulating current, and this is probably effected in some measure through the medium of the secreting organs, in the same way that an effort is made to remove tartarized antimony, when we inject this or any other noxious agent into a vein.

During the cold stage the blood is chilled, the heart is paralysed, there is a state of torpor in the whole system, but particularly in the vascular organs, which is directly the reverse of inflammatory action; most of the secretions are diminished, the appetite is lost, the patient is restless, the animal heat is not evolved in its usual quantity, and the temperature of the body soon becomes much lower than it is in health: but during the hot stage there is increased action in the whole of the vascular organs; the circulation becomes rapid in the extreme vessels, and the temperature of the blood, which was lately so cold, is now augmented, even far beyond its natural standard of 98°. These are the effects; *but a vitiated state of the blood, producing functional disease in all the solids, derangement in all the secretions, and sudden variations in the temperature, not merely of a part, but in the whole system, is, as I believe, in every instance, the very essence of fever.*

I am well aware that I have been treading on debateable ground, and that many objections will, probably, be made to the above theory. Those that are advanced in years do not easily become converts; and the young will be slow in adopting any doctrine that is not yet taught by their own teachers; while others, who have already made up their minds, are but too apt to consider every one to be in error who does not think exactly as they do on this subject. As yet, however, little has been done, and ages may still pass away ere the profession arrive at anything like universal belief with respect to the immediate cause of fever; but this, the most important and the very basis of all our doctrines, ought not surely to be left, either to the unfounded assumptions of the solidist, or the idle dreams of the enthusiast. To arrive at truth, we must lay aside all previous prepossessions, and look calmly, but closely, at the disease itself; we may then form our opinions from the facts which we observe, and whatever conclusions we may ultimately draw with respect to the theory, yet should it be found that, in fever, the practice of those who attend to the whole system is more successful than that of the solidist, who looks merely to a part, we shall then, in the mean time, have a solemn reason for believing that the blood may be diseased in fever as well as the solids, and that this vitiated condition of the whole vital current ought not to be entirely overlooked in the treatment of this, by far the most important of all the diseases that we are called upon to contend with.

Those who believe that fever is merely the effect of a nervous impression, and try to cure it by means of sympathy, may not always fail in their practice; and those who adopt the belief that fever is merely the accidental effect of a local inflammation, and attend in their treatment merely to the reduction of increased action, may probably succeed in curing the milder fevers of more northern countries; and even in the less severe cases within the tropics, the patients did not invariably die under the old practice. But it is a fact, that in the more malignant forms of the same diseases, we were most miserably unsuccessful, so long as we trusted to sympathy, or attended only to the solids; for, at that period, we had no command whatever over the disease, and the most successful practitioner was decidedly the one who did least. We were afraid to act, for experience had taught us, that the remedies in common use were not merely of no value, but that most of them were actually destructive to the patient.

The marsh fever, such as is commonly met with in the West Indies, is seldom fatal; for though the blood is under the influence of a morbid poison, yet this is not nearly so virulent as that which produces the African typhus. Still in the first stage of even this most malignant disease, when the excitement was great, we knew what to do; but, in the last stage, our task was, in general, a mere 'melancholy attendance upon misery;' for, as I have formerly said, the oldest and the most experienced physician had, but too often, the mortification of seeing his unfortunate patient bound and struggling in the iron grasp of a giant disease, not only with the belief that he could do him no good, but with a thorough conviction, that most of the remedies which we had then tried, so far from being of any use, decidedly increased the very evils that they were given to relieve; and, in most cases, in place of lessening the suffering, or diminishing the mortality, my belief is, that they hurried the patients rapidly on to their silent abode. But the result was afterwards very different: for from the moment that we abandoned the doctrine of the schools, profited by our own experience, and attended in our practice to the diseased state of the blood, as well as to the solids, we had then a star to guide us in our practice; for, even in the worst cases, when we saw them early, and before any serious injury had been done to any of the solids, we had then, in general, a command even over the worst cases, that is, when we saw them early and before any serious injury had been done to the solids. Since that period we have been able to prevent much suffering, to save many valuable lives; and in those islands where this treatment has been *fairly tried*, those fevers that were formerly so fatal have been, in a great degree, disarmed of their terrors.

Dr. Hunter's work 'On the Diseases of the Army in Jamaica,' was, at one time, considered almost as a text-book, particularly by the younger practitioners in the West Indies. Dr. Hunter, however, during his short residence, probably saw but few cases of the climate fever, and certainly not one case of the African typhus. He considered what had been called the yellow fever as merely the aggravated form of the marsh remittent, and condemned the free use of the lancet, even in the most inflammatory cases of the West-India fevers. This, however, is one of the many evil results arising from the improper system of grouping three fevers into one; and from the many advocates which Dr. Hunter has had, particularly amongst the younger practitioners in the West Indies, I believe from this, and for other reasons, that his work has been productive of positive harm. In speaking of blood-letting, he observes, 'In such 'cases as seemed most to require it, for example, where 'the patient was young, strong, of a full habit, and 'lately arrived from Europe; where the pulse was 'quick and full, the face flushed with heat and head-'ache, and all these at the beginning of the fever, 'bleeding did no good.' Now, from what I have seen, my belief is, that this one sentence, together with the observations with which it is followed up, has cost more lives amongst the soldiers in the Western world, than were lost on both sides on the field of Waterloo.

Dr. Jackson, who has also written on the fevers of Jamaica, was educated at a period when, unfortunately for science, the fluids were completely neglected, and nothing was thought of, in the medical profession, but the brain, and impressions which were supposed to be made on this organ, entirely through the medium of the nerves. Jackson, however, saw some of the objections to the theory of Cullen, and formed one of his own, that was equally exclusive. He tells us that the cause which produces fever 'is a violence.' That the re-action which follows the application of this cause is also 'a violence.' This 'constitutes an 'ostensible disease which is unnatural and contingently 'destructive.' He therefore considers fever as a morbid act, which can only be cured by an action, or 'certain means of force,' which he recommends for the purpose of arresting the febrile act. Consequently, where the excitement is great, he commences the treatment, very properly, by the free use of the lancet*. This is generally essential in the beginning of the disease: but mere bleeding will not cure fever, unless it be followed up with proper subsequent treatment. Immediately after the first bleeding, Dr. Jackson immersed his patients in a warm bath, and the moment they came out of this a severe shock was given to the system, by dashing cold water over the whole body. This I believe to be a very questionable practice: it has lately, however, been proved, on a large scale, that even this will not prevent their recovery, provided the patients be properly treated in the subsequent stages of the disease.

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* Dr. Jackson, however, was by no means the first who used the lancet as a remedy in the West-India fevers. Warren, who published on the African typhus, or Malignant fever, in Barbadoes, in the year 1741, recommends bleeding in the first stage, and condemns the use of mercuriál preparations. But, besides this, bleeding had been used in Jamaica before the arrival of either Jackson or Mosely, and a pamphlet had been published in that island, by Dr. Spence, recommending bleeding in the West-India fevers, as far back as the year 1776.

Dr. Jackson generally ordered a purgative immediately after the shock; but he preferred the milder purges to those that were more active. The purgative that he frequently used, consisted of calomel, James's powder, nitre, sulphur, and soda. These ingredients were made into a bolus, and given to the patient every four hours. An infusion of senna with aqua ammoni acetat. was given soon after, to assist the operation of the other ingredients. Another purgative, which he frequently used, consisted of calomel and James's powder. This was given without the addition of the other medicines, and followed up with tincture of aloes, combined with one drachm of æther, or one ounce of rectified oil of turpentine.

I do not know on what principle he used æther and turpentine in the beginning of fever. It is, however, in the last stage of the disease that I consider the practice of Jackson as decidedly objectionable. When the stomach is irritable, as it generally is in all the fevers from poison, it will naturally be rendered much more so as the disease advances by the calomel, James's powder, turpentine, &c. &c., which the patient has been using from the very beginning. Under such circumstances, we need not be surprised that extreme irritability of the stomach should be a frequent symptom in patients who are treated in this way: whilst 'the means of force' which Jackson recommends afterwards, are, in my mind, anything but well calculated, either for lessening the irritability in the gastric organs, or curing the disease. 'If the 'tongue be moist and foul, the pulse free and elastic, 'the skin moist and animated,' emetics are then to be used; but he prefers white and blue vitriol to ipecacuanha. If, on the other hand, 'the tongue be rough, dry, and red, or smooth, red, and dry,' especially when accompanied with thirst and sensations of distress at the stomach, he then gives *sugar of lead* and crystals of tartar. Camphorated mixture and white vitriol are also used in similar cases, that is, when the irritability of the stomach is very great, which, indeed, it must be when its structure is almost destroyed by the repeated doses of calomel*, James's powder, white and blue vitriol, but particularly where the patient is poisoned at the same time with sugar of lead. And, as a cure for all this, he recommends four grains of either white or blue vitriol, combined with an equal quantity of ipecacuanha. Now is not this doing, with a witness, what Jackson calls '*taking the business as* '*speedily as possible totally out of the hands of* '*nature?*' He tells us, however, that the unfortunate course of the fever appears to be sometimes suspended, in the last stage, as if 'by a stream

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* I was told in America, that a physician in Norfolk had given considerable doses of calomel to inferior animals, which were killed, about an hour after, by a blow on the head. It was found that every part of the stomach was inflamed which this insoluble substance had touched: and this I can readily believe, from having often witnessed the distressing symptoms which are produced when this agent is used, particularly when the stomach is so irritable, as it generally is, in the fevers from poison. Rush states that griping and pains in the bowels, amounting to colics, were very common in his patients; and this is probable enough, considering the large quantities of calomel which he used.

'of life thrown into the system at those times, in a 'manner as cannot be explained, but that tends, by 'the new action produced, to avert death.' I am at a loss how to account for the occurrence of this, in any one case where the patients are treated in the manner recommended by Jackson, except by supposing that they were occasionally saved by the use of the Seidlitz powders, which he sometimes accidentally used; for he confesses that he had seen the most beneficial results from these powders, and that these, or milk, were the most effectual remedies in allaying the irritability in the stomach, which must have been so distressing where the patients were so unmercifully treated with calomel, James's powder, vitriolic acid emetics, and sugar of lead.

In addition to the above measures, Dr. Jackson used opium*, which we know to be a fatal medicine in the African typhus. Valerian, and the acid of amber, were also occasionally given. He bled, in some cases, late in the disease; and bleeding is now well known, in the West Indies, to be as injurious in the last stage, as it is useful in the first. He believes, also, that the moon has an influence over fever, and talks about the system being occasionally saved by the blowing up of certain organs. In addition to his other acts of force, he seems to place great reliance on what he calls 'infusing life into the system,' by the application of red-hot irons to the back of the neck, applied on each side of the spine, and as near to the brain as possible.

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* I have never known one case recover from the African typhus, where the patient took a full dose of this medicine in the last stage. Such is the fact in the West Indies, as well as in other parts of the world. When this disease was raging in Philadelphia, in 1793, opium was tried and found to be most injurious. Mr. Conolly states, that 'in some cases, where the chain of the disease appears to have been broken by other remedies, and where 'the patients were in a fair way of recovery, a single dose of laudanum has hurried them suddenly into eternity.'

Such is an outline of the treatment recommended by Jackson; and those who have had an opportunity of seeing the fevers that he had to contend with, and know what his practice was, will not be surprised at his own confession, that a road was still left open, not merely for improvement, but almost for total innovation, in the treatment of, what he calls, the yellow fever.

Dr. Jackson, however, was by no means destitute of talents. He was fond of his profession, and fearless of danger, whenever he thought that he could do good to his fellow-creatures. In the year 1819, he went to Spain, at an advanced period of life, with the laudable intention of making some inquiries relative to the epidemic which was then so destructive in that country; and those who are acquainted with the means that were used by the Spanish physicians, will not be surprised at the dreadful mortality. Their chief remedies were emetics, opium, large doses of bark and mineral acids; by the use of which, even the mildest were converted into malignant cases; and though the African typhus is a milder disease in Europe than in hotter climates, yet those who were treated by the Spanish physicians seldom recovered.

In speaking of the cases which occurred in the hospital of San Juan de Dios, at Cadiz, Dr. J states, that 'the disease did not in general appear to 'be violent at the commencement: it was treacherous 'in its movements, and often proved fatal unexpect'edly.' Had Dr. Jackson, however, been acquainted with the effects which their remedies produce, he might easily have explained why even mild cases were suddenly rendered malignant, and why the Spanish treatment, in this fever, was so miserably unsuccessful, not merely in Cadiz, &c., where he had seen it, but all over Spain. Jackson admits the fact, that the Spanish physicians were most unsuccessful, and candidly confesses, also, that even in Spain *he could not do much more for the patients, with his own treatment, than the Spanish physicians had done with theirs.* This I believe to be the honest truth, for the practice of the one was founded almost entirely on an erroneous opinion with respect to the blood; whilst that of the other was the result of an unfortunate theory with respect to the nature of fever, and a belief that it could be cured by remedies which produce their effect entirely through the medium of the nervous system.

I can scarcely believe it possible for any patient to recover from a malignant fever, who is treated in the manner recommended either by Dr. Jackson or the Spanish physicians. The practice of the former may be good in the beginning of the disease; but what he had gained in the first stage, he lost in the last by bleeding, acrid emetics, and sugar of lead. This latter agent is a most improper remedy in any fever, whilst bleeding is invariably fatal in the last stage; and afterwards I may bring forward some melancholy cases to prove the evil effects of acrid emetics in the African typhus. This, however, is nothing new.

Warren, who published on the African typhus nearly a century ago, observes, 'As to emetics: 'These come usually next in order, and the unfortunate patient must, immediately after bleeding, 'have a vomit crammed down his throat, especially if he shows any signs of sickness of the 'stomach, or oppression about his breast; even the 'by-standers will usually solicit hard for it, and a 'modest man may not always have firmness enough 'to resist all their clamorous instances. I have too 'often, by experience, found, that vomitives enrage 'the distemper, raise an unappeasable hurry and 'tumult in the spirits, grievously affect and irritate 'the nerves of the *plexus cardiacus*, and all the nervous membranes about the præcordia, give a further 'dissolution to the blood, and, in short (if I may use 'the phrase), raise a dæmon in the system, that is 'scarce ever after to be laid, but by the death of the 'patient. I have, indeed, sometimes seen the sick 'man escape with much difficulty through this danger; but, generally speaking, the effects of such 'an administration in this distemper are so dismal 'and surprising, that the stomach will receive neither 'medicine nor sustenance for the future, until after the 'most laborious and painful concussions, hiccups, 'and almost convulsive contractions of the parts, signs 'of inward gangrene discover themselves, and nature's 'strength is at length quite spent and worn away.' Such I believe to be the fact; yet there are still practitioners who, when they have a favourite theory to support, will neither profit by their own advantages, nor receive any benefit from the experience of others.

In the treatment of fever, in order to be able to prevent the mortality, it is necessary for the physician to have a correct idea of the nature of those morbid changes by which the disease terminates in death, for those who do not attend to this may prescribe for symptoms, but they can never strike at the root of the evil, and if they do good, it is only by chance. Now, in as far as relates to the West India fevers, death may be produced in one or other of two ways. In the first place, it may be the effect of injury done to the brain or some of the other important organs during the violent excitement, which is so common in the inflammatory stage of these diseases. This, however, in the present day, is but very seldom the manner in which fever runs its course to a fatal termination; for mere excitement is much at the mercy of the lancet, and this cause of death is so obvious, that it can almost always be easily prevented. Up to this period the practice is clear enough; but, after this, all is darkness and confusion to the physician who does not attend to the vitiated condition of the blood. Where there is great excitement, the mere reduction of the increased action is essential to the safety of the patient; but this of itself, though it may cure inflammation, yet it will not cure such fevers as we have frequently to contend with in the West India islands: for under the common treatment, of attending merely to the solids, the disease, in the last stage, runs rapidly on to a fatal termination. Even when left to itself the fever abates, but the danger increases; and though, in some of the worst cases, there are no symptoms of organic disease in any of the solids, yet the feelings of the patient are infinitely more distressing than those that are produced by mere pain. The heart is now evidently labouring to propel the black and vitiated blood with which it is gorged; there is a want of action in the extreme vessels, and coldness particularly in the extremities; there is, as the patients sometimes express it, a sinking from within; they know that they are very ill, and feel that they are dying, but they cannot tell why. They become exceedingly restless, the colour of the skin suddenly changes to a dark dirty yellow, and in some cases, it is almost black. The breath becomes fetid, the secretions entirely cease, the excitement is reduced, but the distressing feelings become rapidly worse, and the patients die, sometimes convulsed, but in other cases they cease to live almost without a struggle, and that too, at a time, when those who overlook the morbid condition of the blood cannot assign any sufficient cause for the sudden death.

Now, what is the cause of death in such cases? The patient does not die from excitement, for that, God knows, even where it exists, is generally more than sufficiently reduced. But when we see the black and diseased blood so thin, that in some cases it issues from the eyes, the tongue, &c. &c., even before death, it is then evident th the whole vital current is undergoing sudden and fatal changes, which (*if not prevented*) will soon render the vital fluid totally incapable of stimulating the heart, the brain, or any of the other organs, and, consequently, quite incapable of supporting life.

When we examine the body, in such cases, after death, we find the heart flabby in its consistence, and crammed with a thin, black, half putrid fluid, that can only be distinguished from the black vomit by a chemical test *. Now, when we find the solids free from any organic derangement, and the blood so much diseased, we are naturally led to infer that this was the true cause of the fatal event; and, in fact, in the malignant fevers of the West Indies, this is, by far, the most common cause of the mortality. But how are we to prevent this? Certainly not by fretting an *acid* and irritable stomach with large and repeated doses of vitriolic acid emetics, calomel, antimony, sugar of lead, &c. &c., as recommended by Jackson; neither can we remedy the diseased state of the whole circulating current by applying red-hot irons to the nape of the neck on each side of the spine, and as near to the brain as possible, as Jackson declares he would have done, if he had had charge of a military hospital, where he had nothing to interfere with him but his own conscience.

Opium is what it has been called by Mosely, 'a fatal medicine,' for, as I have said, those to whom it is given in full doses never recover, particularly in the last stage of either the climate fever or the African typhus. Nor can we prevent the mortality by lessening, like Broussais, the quantity of the blood with innumerable leeches*, at a time when it is our imperious duty, not to lessen the quantity, but to improve the quality of that fluid, the healthy condition of which is so essential to life. And when we know that its black colour, in the last stage, as well as its other diseased properties, are produced chiefly by the loss, or, at least, by the great diminution of those natural saline ingredients which are so essentially necessary to the healthy condition of the vital fluid; and when we know, also, that if we add a small proportion of saline matter to this black and half putrid blood, even out of the body, that we not merely restore its red colour, but retard, for a time, the process of putrefaction—is it not, then, under these circumstances, consistent with common sense, as well as experience, to believe, that we should best benefit our patient, by throwing into the circulation those natural and essential salts which the blood has lost? If we continue to bleed the patients, under such circumstances, they will be sure to die; or, if we fret the stomach, at this period, with large doses of vitriolic emetics, calomel, or sugar of lead *, they will never recover, at least in those cases that are really severe.

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* The matter of black vomit instantly reddens litmus paper, and effervesces freely with the carbonates. This, however, is not the case with the black blood which is taken from the heart.

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* It is asserted, in a late number of an American journal, that in the science of Medicine, the English practitioners are, at least, half a century behind their continental neighbours; and, to support this, the theory of gastro-enterite is brought forward as a certain proof. M. Broussais approves highly of this opinion, and informs us that it is indeed a great truth. He affirms, also, at the same time, that it is only national jealousy which prevents the English physicians from opening their eyes to the great beauty of his doctrine.

In a lecture which was published, in 1828, Dr. Clanny states, that in typhus fever, as the disease advances, there is a progressive diminution of all the solid ingredients, and an increase of water in the blood. He does not, however, appear, either in his theory or practice, to have had the slightest idea of the value of the active salts, which invariably exist in the vital current, or of the consequences which must inevitably follow, when these are diminished, either in typhus, or in any other disease; and, whatever importance we may attach to his analysis of the blood in typhus, yet his theory of fever is decidedly in direct opposition to his own facts, and his practice is not merely the most trivial, but decidedly the most unscientific that has yet been proposed.

We have seen that carbonic acid is the cause of the impurity in the venous blood, and that the lungs are formed, almost expressly, for the purpose of removing this source of impurity from the venous circulation. We know, also, that when we breathe pure nitrogen, or any other air that does not contain oxygen, the carbonic acid is not removed, and it then acts as a poison in the left side of the heart. Yet, it is this poison that Dr. Clanny considers as the main-

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* If such remedies can cure fever, why do they not in the last stage, or in bad cases, inject these agents into the veins? The injection of *saline fluids* directly into the blood has been found to be useful in cholera, and my belief is, that this may be used with advantage in hydrophia, asphyxia, plague, and all other diseases where the vitiated condition of the blood is the chief cause of death.

spring of life. He believes that the diminution of this is the chief cause of the diseased condition of the blood in the last stage of typhus fever, and his practice consists, not only in distending the stomach with this gas, but he actually invented an apparatus for throwing large quantities of carbonic acid into the intestinal canal. Now, it is very evident that if this gas be absorbed, or attracted into the circulation, it must first mix with the venous blood and add to its impurity. Fortunately, however, it is either not absorbed, or, if it be, it is generally removed in the lungs before it can do any mischief: for, if it were possible for this gas to enter the arterial circulation, so far from being the main-spring of life, it would act as a poison and cause instant death. This, however, can only occur when there is such an excess of carbonic acid in the circulation, that the oxygen in the lungs is not able to attract the whole of this deleterious gas from the venous blood, during its rapid passage through the pulmonary organs; and this, in fact, appears to be sometimes the case. 'On sait, d'après les recherches de Jurine, de MM. Chevreul, et Magendie, et d'autres, que ce gaz existe dans presque toute l'étendue du canal digestif. On ne peut refuser d'admettre qu'il s'en forme dans le travail de la digestion. En contact avec presque toute la surface muqueuse du canal digestif, une partie de ce gaz doit être absorbée. Si l'on en doutait, je citerais des cas où de l'eau, impregnée d'acide carbonique et bue en quantité suffisante, a produit des symptômes d'asphyxia. Le docteur Desportes a communiqué des observations à ce sujet à l'Académie Royale de Médecine*.'

The mercurial treatment has now been fairly tried in the various fevers of the western world, and certainly it is no recommendation to this practice, that Chisholm, Rush, and others who have used it most, have been, in their practice, decidedly the least successful, with the exception only of those who have adopted a false theory with respect to the blood, and believed that the red colour of this fluid, when it became black, could be restored by the use of acids, and other agents that contain oxygen in a separable form. Some other modes of treatment have done much mischief; but this has been the most destructive of all: for the truth is, that what they wanted to effect by oxygen, can only be done by the neutral salts; and the acids, though they redden the colour of the vegetable blues, yet, like the febrile poisons, they interfere with the agency of the saline matter, and blacken the colour of the blood, even out of the body; and when the stronger acids are used internally, in malignant diseases, they hasten the fatal symptoms, and cause almost instant death. On the other hand, we are often successful, even in the worst cases, when we bleed, as much as is necessary, early in the disease, give purgatives in the beginning, such as the castor or Croton oil; or even calomel may be used as a cathartic in the first stage of the climate fever. It is also essential to remove, at the same time, the morbid heat from the body, by the constant use of a sponge frequently dipped in cold water.

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* Influence des Agens Physiques sur la Vie, par W. F. Edwards.

By the early and judicious use of these means, we can reduce the excitement, and prevent any serious injury from being done, either to the brain or to any of the organs. But still the patients will die, if we continue, after this, to trust either to these means, to mercurial preparations, opium, acids, antimony, brandy, or any of the other remedies which have been formerly in general use; but they will probably recover, if, after having properly reduced the excitement and protected the solids, we now attend to the diseased state of the blood: for at this period, in place of lessening the quantity, it is necessary to remedy its diseased quality, and, instead of fretting the acid and irritable stomach with improper and unscientific remedies, we must now correct the acidity, which is, in reality, in the last stage, the chief cause of the great irritability, as well as the intense burning in the gastric organs. It is in such cases that the alkaline carbonates are of so much value; and then, when this is effected, it is necessary to throw into the circulation an extra supply of those stronger salts which we know do enter the circulating current almost immediately by the vena portæ, and act directly on the blood*. When these are used at a proper period, they not only redden its colour, but add to its power of stimulating the heart. They correct the diseased properties of the vital current, and generally prevent those bad symptoms which are so commonly met with, when these remedies are not used. We know, also, that the stronger salts pass into the circulation without either being decomposed, in the gastric organs, or changed in their properties by the vital principle. As much as is necessary is retained in the circulation, and the excess is thrown out by the secreting organs, which do not cease to secrete when a proper supply of the active non-purgative saline agents are thrown into the system sufficiently early. It is also proper, in the last stage, to support the strength of the patient with beef-tea, or some other rich, but clear, well-seasoned soup. The former will correct its diseased properties, and the latter will soon be converted into chyle, so as to supply the current with the proper *matériel* for the speedy formation of new and healthy blood.

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* Those who have any doubts relative to the important effects which can be produced, even by very minute portions of active chemical agents, may consult, with advantage, the valuable letters of Dr. Prout, contained in the late numbers of the Medical Gazette.

It is very probable that poisons which are attracted into the circulation and mix with the blood, ultimately produce a morbid impression on the nerves of the internal surface of the heart, &c. It is also not improbable, that the narcotic effect of the poison may render those surfaces less sensible to the effect of stimuli; but still the blood is the medium through which the poison acts upon the solids: for this is the first part of the system which feels the effect of the remote cause; and when the blood is poisoned, the impression which is ultimately made on the nervous system is merely the effect of the diseased state of the vital current, which, as I have said, receives the shock, and communicates the disease to the whole system. Now, if the cause of the disease produces its effects entirely through the medium of the blood, we shall then be most likely to effect a cure by the use of those agents which enter the circulation, and, like the remote cause of fever, act not only on the blood itself, but on every organ and every solid of the living body, not by any direct impression on the nervous system, but entirely through the medium of their nutritive fluid.

Where the practitioner is called in to a patient, in the very last stage of fever, I believe that life may occasionally be saved by injecting a saline solution into the veins. This, however, is seldom or never necessary, when we see the patients at a period when the stomach and intestines are still capable of performing their functions, which they frequently do to a very late period of the disease; for in many instances, even where we have been called to the patients in the last stage of fever, I have seen some most malignant cases cured by the internal use of the alkaline salts, which most assuredly would have proved fatal under the common practice of the mere solidist. Now, as we know that the active salts enter the circulation, and produce their beneficial effects in fever, almost entirely through the medium of the vital current, from this alone we may infer that the malignant symptoms are the effect of a vitiated state of the blood; for if these were produced only by inflammation, or some organic derangement in the solid structure, this disease could not be successfully treated merely by the use of active stimulating remedies, which produce their effect, not only by reddening the blood, but by correcting the diseased properties of the whole vital fluid.

For a considerable period after I commenced the saline treatment, I used (except where there were symptoms of acid in the stomach) a strong solution of the muriate of soda with nitrate of potass. I have since employed the nitrate of potass less frequently, not from any want of success, for we did not lose one case where we were called in at a proper period of the disease; but I have since found that the chlorate of potass and other active saline agents answer the purpose equally well, and some of them have this great advantage, that they do not fret the stomach, even in the slightest degree.

We have seen that the muriate of soda is the chief saline ingredient in the blood. We know, also, that the quantity of this is greatly diminished in the last stage of malignant diseases; and I have seen some patients, in the very last stage of fever, recover under the internal use of large doses of this and other active saline agents, where the cases at first were so hopeless, that their recovery afterwards appeared to be almost a miracle.

I will afterwards prove that there is no part of the world where the practice in fever is so bad as in Spain, or where, in proportion to the number of cases, the mortality has been so great from the African typhus. During the last epidemics, however, some of the common people, at Alicante and Carthagena, having very naturally lost all faith in the medicines which were used by the physicians, tried sea-water with great success. Monsieur Bally, one of the French commissioners who were sent to Spain during the epidemic of 1821, states that they met a capuchin at Orihuela, who told them that he had been attacked with the fever, and had cured himself by the internal use of sea-water; in consequence of which he had recommended it to others, and that all of them had recovered. The commissioners, however, merely state the fact; but as the result did not correspond with their own theories, they neither tried it themselves, nor recommended it to others [*].

Arejula, who has written largely on the fevers of Spain, point blank condemns the mercurial practice in the African typhus, and states that a physician who was sent by the Cortes, during the epidemic of 1804, began by bleeding; he then had recourse to mercurial frictions, and the internal use of mercurial medicines; but, says Arejula, 'I am very sure that 'he will not publish the effects of this destructive 'treatment [†].'

Arejula, and his followers, avoided the error of fretting the stomach with mercurial preparations, but they fell into another, so very injurious, that those patients in Spain were decidedly the best off who had least to do with the regular practitioners; and, perhaps, not one would have escaped the destructive effects of the acids, brandy, opium, and other pernicious medicines, with which they were treated, had it not been for the administration of the sea-water, which was thrown up frequently into the intestines. Arejula acknowledges that this was of great service, *and not to be omitted on any account;* and to this, in all probability, the few that escaped owed their recovery.

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[*] See Typhus d'Amérique, par Mons. Vr. Bally, p. 545.

[†] 'Pero estoy bien seguro que no publicará los efectos de este método destructor.'—See Arejula, p. 216.

When the nitrate of potass is mixed with dark blood out of the body, it possesses, like the other salts, the power of communicating an arterial colour instantly to this dark fluid. We know, also, that nitre, like the other salts which are formed with the mineral acids, does not undergo any decomposition in the gastric organs, but passes through the circulation in its pure state. When this is given in diseases where the blood is black, as it always is in the last stage of fever, a part of the nitre will remain in the circulation for a time, and produce the same change, both in the colour and the properties of the blood, that it does when mixed with this fluid out of the system.

It has long been known that the nitrate of potass was a valuable remedy in the bad forms of typhus fever; but, as its modus operandi was not understood, it lost its reputation, from having been used at the same time with other most improper medicines. Many years ago, it was also found to be a valuable remedy in scurvy; and, lately, it has been tried with great advantage in that disease, by one who had no theory to support, and who used the nitrate of potass only because it had been recommended by another, who had found it to be useful, when given to the sick in this disease.

The Ferguson transport, bound with convicts for New South Wales, sailed from Ireland in the month of November, 1828. During the voyage, the scurvy broke out; it was very bad amongst the convicts, and even 'threatened to depopulate the crew.' Mr. Cameron, the surgeon of the vessel, states that he had recourse to the use of nitre, in large doses, and that its effects were most miraculous.

I may observe, that Mr. Cameron (as I believe most injudiciously) used vinegar along with the nitrate of potass. This was, probably, given from the old, but erroneous belief, that acids oxygenate the blood. If Mr. Cameron, however, had tried the effects of the vinegar and nitre separately on the black blood out of the body, he would not have used them both together in the same disease; for if the one be proper, the other must be directly the reverse. He also states, that two years before he had used the *nitre by itself*, in some severe cases of scurvy, and with a similar effect in arresting its progress.

'I might add,' says Mr. Cameron, 'that the most 'distressing symptoms which my patients complained 'of in the early stages, namely, a sense of oppression 'and sinking at the pit of the stomach, were almost 'invariably relieved, or totally removed by a few 'doses of the medicine. The prisoners themselves 'were so sensible of its good effects, that I had, for 'the first time, an opportunity of seeing men crave 'for medicine, the taste of which was certainly not 'pleasant; and their complexions were so much 'improved under its use, changing from a sallow 'bloated hue, sometimes approaching to livid, to a 'clear, healthy colour, that it became a matter of sur'prise to every one*.'

The muriate, the carbonate of soda, the chlorate of potass, and the other neutral salts, have precisely a similar effect in the middle and last stages of fever. They relieve the bad feelings, the skin becomes clear; and when given before the stomach has ceased to perform its functions, the bad symptoms soon disappear. In such cases, those salts that do not fret the stomach, or operate too freely on the intestines, but enter directly into the circulation, are, as I believe, by far the most valuable remedies in the whole range of the materia medica.

I was informed by Dr. Bardenheuver, a physician of high respectability at Aix-la-Chapelle, that some Dutchmen were brought there a few years ago, who had long been afflicted with a most inveterate form of scurvy, which had resisted the effects of, or more probably the disease had been kept up by, the citric and other acids, which had been used extensively in these cases. They were all of them, however, cured in a very short time, by the natural non-purgative saline waters of Aix.

There is a paper on scurvy, contained in the Medico-Chirurgical Review for June, 1824, in which it is stated:—'In the year 1822, his Majesty's ship 'Leander sailed from Trincomalee for the Cape of

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* See the Medico-Chirurgical Review, March, 1830, p. 483.

'Good Hope, taking on board the mechanics of the 'dock-yard establishment then reduced on the 'island. There were also embarked twenty-six 'invalids, and all the sick that could be removed 'from the hospital. These invalids and sick were 'principally affected with chronic hepatitis, dysen'tery, and phthisis pulmonalis, all of which (even 'some who were expectorating large quantities of 'purulent matter) recovered on the passage to the 'Cape. This good fortune was counterbalanced by 'scurvy, which broke out among the crew, *and in 'spite of large quantities of lemon-juice plentifully 'administered,* in conjunction with every other anti'scorbutic which the ship could produce, spread to 'an alarming extent, and, in one case, proved fatal. 'Had they not reached the Cape at the time they 'did, the Leander would have presented as deplorable 'a spectacle as the Anson, at Juan Fernandez, not'withstanding the supposed specific *lemon-juice,* which 'in no instance, on board the Leander, had the *slight'est effect in even checking* the ravages of scurvy. 'Immediately the ship reached the Cape, and the crew 'got plenty of fresh *animal* food, in conjunction with 'vegetables, they rapidly recovered. Specimens of 'the lemon-juice were transmitted to the Victualling 'Board, and carefully analyzed in London. It was 'found perfectly good *.'

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* An American sloop of war, the Vincennes, Captain Finch, returned last year from a voyage round the world. She had been out about four years: they did not use one particle of citric or any other acid as a preventive, and during the whole voyage they had not one case of scurvy on board the vessel.

Nature does nothing in vain; and for a wise purpose she has provided, in every country, innumerable spots where there is a natural and constant supply of saline waters. These are resorted to by the sick, in all parts of the world; and experience has proved that in diseases of debility, such places are in general the true fountains of health. Some of these waters, like those of Cheltenham, Epsom, Saratoga, &c., owe part of their virtues to their purgative quality; whilst others, such as the saline waters of Ems, Aix-la-Chapelle, &c., are not in the slightest degree purgative, and owe their virtues entirely to their effects on the blood, for they all increase its arterial colour, and add for a time to its stimulating power.

Dr. Bardenheuver told me, that during a residence of forty years at Aix-la-Chapelle, he had invariably found, that the saline waters of Aix were highly useful in all diseases where there was a want of action, but equally injurious in all cases of either increased excitement in the whole system, or acute inflammation in particular parts. In my own practice, I have often seen the exciting effects of even the saline purgatives; for in those cases where they did not operate freely on the intestines, I have seen them produce general excitement, and in some cases, such a determination of blood to the head, that I was obliged to use the lancet to lessen the increased action. This effect, however, is particularly apt to be produced by those saline medicines that are not purgative. I saw one case at Ems, in 1829, where a person in health, after using the waters for three days, was attacked with violent throbbing at the heart, flushings in the face, and bleeding from the nares. In fact, the exciting effect of the saline waters is so well known to the intelligent practitioners in such places, that where there is anything like plethora in the system, they never allow even the purgative saline waters to be used, until the patient be first reduced by the lancet, &c.; and those who neglect this are very frequently injured by their use. While at Saratoga I was informed by a physician of that place, that he had seen several fatal cases of apoplexy in patients who had neglected this precaution. All the saline waters, whether natural or artificial, possess a similar effect in increasing the action of the vascular organs: in those that contain iron, the exciting quality has been attributed to it; but in most of them the quantity of this metal is very minute, and those that have not one particle of iron, redden the blood, and excite the whole system fully as much as those that possess chalybeate properties. The waters, for example, of Aix-la-Chapelle do not contain one particle of iron; yet their exciting and diuretic effects continue for two months after their use has been discontinued.

The salts are by far the most essential ingredients in the circulating current, but 'though the 'serum and crassamentum did not pass entirely 'unnoticed, the red globules were the part of the 'blood which first excited the attention of physi-'cians, and seemed to promise a rich harvest of dis-

'coveries; a promise which too surely has never 'been fulfilled. The red particles have always ap'peared important, because they seemed to give the 'colour, the useful qualities, and the whole charac'ter to the blood *.' But notwithstanding the great attention that has been paid, in all ages, to the colouring matter, it is well known that this is the ingredient of all others that is least essential in the blood. In some cold-blooded animals, the whole circulating current is entirely destitute of hæmatosine; and in the transparent parts even of the human body, the vessels are stimulated and the solids are nourished by blood that has no colouring matter. In most kinds of fish, the red blood is confined to the heart and large vessels, while the clear serous or saline part of the current is sent into the smaller arteries to nourish the solids; and even in vegetables, where there is neither brain, nerves, nor hæmatosine, a clear nutritious fluid circulates in the vessels, and performs all the functions of red blood; but this clear fluid invariably contains a given proportion of certain salts, similar to those which exist in the blood, and, so long as the sap † circulates, it enables the

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* John Bell.

† Some of the stronger salts are absorbed even by vegetables, and enter unchanged into the circulation of the plant. It is well known, that the sap as well as the wood of those plants that grow near to the sea, contain salts of soda; whilst those that grow in the interior of all countries, contain the salts of potass. The juice of the sugar-canes that grow very near to the sea, is sometimes so intensely salt as to render the sugar almost use-

plants, even in cold weather, to keep up a temperature higher than the surrounding medium. In the chick in ovo, when artificial heat is applied, a clear saline fluid stimulates the heart into action, long before the colouring matter begins to be formed: and, in all those animals where the circulating current is colourless, a clear saline fluid stimulates the heart, nourishes the body, endows it with life, and serves all the purposes of red blood. From this it is evident, that the colouring matter is only a secondary agent in the nutritive fluid, but the saline portion is so essential, that blood is never found to exist without it; and when this is nearly lost, as in the last stage of the malignant fevers, the blood soon ceases to possess the power of supporting life, for blood without salt can no more stimulate the heart or enable the solids to perform their functions, than air without oxygen can redden the blood out of the body, or purify the vital stream in the pulmonary organs.

There may be an excess, however, as well as a deficiency of saline matter in the blood; for this reason, the active non-purgative saline medicines ought never to be used too freely, either in full health, or in any case where there is inflammatory disease in a part, or violent increased action going on in the whole system. In such cases, by adding to the sti-

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less. It has also been found in America, that when a strong solution of muriate of soda is applied to the roots of fruit trees, it is absorbed, and renders the fruit so salt that it can scarcely be used. This is particularly the case with the peach-tree.

mulating power of the blood, they increase the excitement in the solids, and augment the severity of the inflammatory symptoms.

When there is an excess of acid in the stomach, there are few physicians, I believe, who have not been in the habit of using the alkaline carbonates, not only in fever but in other diseases.

I have also long been aware, that the saline purgatives do not produce debility like most of the medicines of the same class; and, therefore, like many others, I have long been in the habit of giving saline purgatives in fever, particularly after the patients had been properly bled; but I was not then aware of the real value of the non-purgative saline medicines, and combined them with other agents which I know now to have been most improper. When salts that are purgative are taken into the circulation, they excite the system, but the excess is soon removed by the intestinal canal; whilst those that are diuretic are thrown out by the kidneys. When the muriate of soda, and other salts that are not purgative, are taken into the circulation, they excite the whole of the vascular organs, they cause great thirst, and the excitement continues until the blood is diluted with water; the excess is then removed, and, after a time, the increased excitement abates.

If brandy, opium, acids, and other active agents that do not naturally exist in the blood, are added to the still living fluid which is just drawn from the body, they not only injure its structure, but instantly destroy its vitality. When these agents are used internally, they are generally supposed to act by a sympathetic or direct impression on the nervous system; but my conviction is, that most of them enter the circulation, and do mischief, for, as we have seen, some of these agents mix with the blood with great rapidity, and when they do so, they are most destructive.

When used in health, brandy, opium, &c., produce a transient excitement, which is invariably followed by subsequent depression; so that, in the end, little advantage is derived from their use; except in large doses, however, they do not cause death, either in healthy individuals, or in those who are suffering from the milder fevers. But in cholera, or in the last stage of other malignant diseases, where life is trembling in the balance, and where the weight of a straw, in either scale, may decide the fate of the patients, these, surely, are not proper agents to be thrown into the circulating current; particularly at a moment when the blood is already not merely black in colour, but too much diseased. Such, however, is the common and daily practice in malignant fevers; yet neither the innumerable deaths, nor the sudden mortality which almost invariably follow the employment of these destructive agents, can deter the generality of practitioners from the use of medicines which are only given on the faith of a false theory, and which, if they stupify the patients, or lessen the suffering, it is only by more rapidly producing those fatal changes in the system which render the body no longer a fit habitation for the principle of life.

I would not dwell upon this subject, were it not from a firm conviction, that, even at this moment, there are men in the profession who, from being guided by a wrong theory, are daily in the habit of doing those things in the treatment of malignant diseases, which, when they come to see in their proper light, they will think of them with horror as long as they live.

When patients become weak, as in the last stage of fever, it is necessary to keep up the action in the vascular organs. But if we trust to the momentary excitement which is produced by wine, brandy, and opium, we are generally deceived, for, as I have said, this is soon followed by debility; whilst, on the other hand, those active salts which are natural to the circulation, not only redden the colour of the black blood, but are decidedly the most permanent and the best stimuli which we yet possess.

It is now well known that when we inject a small portion of non-purgative saline fluids into the veins of an inferior animal, it reddens the blood and produces violent increased action in the whole of the vascular solids: or if we use them in excess internally, they cause great excitement in the whole system; but when combined with proper nourishment, they are by far the most valuable remedies, not only in fever, but in all diseases of general debility. In chlorosis, jaundice, scurvy, melonosis, chronic enlargement of the liver, dropsy, gangrene, bad sloughing ulcers, and, in short, in all diseases where the blood is in a black and vitiated condition*, the natural saline waters, or the active non-purgative alkaline salts, act like a charm. They enter the circulation, and not only redden the colour of the whole current, but remedy its diseased properties, adding at the same time to its power of stimulating the vascular organs much more effectually than either bark, brandy, opium, or wine. These produce but a momentary excitement, while, under the use of the saline remedies, the vitiated blood becomes more red; they increase the action of the heart, lessen the morbid excitement in the cerebral organs, and when their use is combined with proper nourishment, the strength of the patients increases daily, and thus their health is often restored, when this could not have been effected by any other means.

We have seen that venous blood, which is under the influence of a febrile poison, requires a larger portion of salt to give it an arterial colour, than is necessary on purpose to produce a similar appearance on the same quantity of healthy blood. We know, also, that when the blood is not diseased, and possesses its full vitality, its usual quantity of saline matter is quite sufficient to enable it to perform its functions; but when the whole current is under the influence of a morbid poison, or deranged in its properties from any other cause, an extra portion of saline matter is then essentially necessary, for the purpose of reddening the blood, so as to enable it to stimulate the heart, and support life.

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* I am authorised to state, that a physician of great respectability in this metropolis has been lately, at the suggestion of Dr. Prout, using the saline treatment with decided advantage, in some malignant cases of puerperal fever.

But, independent of their power of reddening and adding to the stimulating quality of the blood, there are also many facts which induce me to believe that some of the active salts possess a *specific* property, by means of which they are capable of neutralizing and preventing the action of the aërial poisons that cause fevers.

Salina is a small town in the Genesee country, situated near to the Onondaga lake. During the hot months, the inhabitants are exceedingly subject to violent and frequently to fatal attacks of the marsh fever. Many of the numerous salt-works near to this town are placed in the lowest part of an extensive swamp; but those persons who are employed in these works are almost entirely exempt from fever. Those individuals are kept in the salt factories six hours at a time, and the other six they are generally out in the open infected atmosphere. Such persons are not only exposed to sudden alternations of heat and cold, but, as the works are in the marsh, they are exposed also to the poisoned air as much as any of the other inhabitants who live in the same sickly locality; yet so long as they continue in their employment as boilers of salt, they remain healthy, at least they are exempt from the various forms of the marsh fever, which is so common, and frequently so fatal, amongst the other inhabitants who live in, or near to, the same swamp.

The water from which the salt is obtained, at Salina, is saturated to excess with a variety of saline ingredients *, and of all the agents which I have ever mixed with venous blood, this is the one which gives it the most beautiful, bright arterial colour. When the water in the boilers is exposed to a high heat, a part of the saline particles is diffused in the air, and some of them again crystallize on the upper wooden beams of the building. Now, may we not infer, that when this air which is impregnated with saline matter is taken into the circulation, the excess of this destroys the poison and prevents its action on the living body? And the fact that this

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* The wooden pipes which are used in America to conduct fresh water, rot in a very short period, whilst those that are used for conducting the salt water at Salina become hard as iron, and look as if they would last almost for ever. It is also a fact, that vessels which ply constantly on rivers, &c., rot much faster than those that sail in salt water; and, probably, the dry rot, which is so destructive in the navy, might be prevented by keeping the ships of war as much as possible in salt water; for it is not improbable that this is often produced by taking them into places, such as the harbour of Plymouth, &c., where there is a mixture of salt with a large proportion of fresh water. But be this as it may, we know that when rain water falls on the deck of a vessel and is allowed to remain, the wood becomes soft and soon rots; but, when this is removed, and the part is well washed with salt water or rubbed with common salt, it becomes harder than it had been before.

air is destructive to the cause of fever is now so well known, that persons who have been long under the influence of the marsh intermittent, are cured by this saline atmosphere in these works, more effectually than they are either by bark, purgatives, or even by large doses of the sulphate of quinine.

When persons who reside in sickly districts are employed, for the first time, in the salt-works, the saline atmosphere makes a strong impression on the system. They generally feel unwell for the first few days, and, frequently, in less than a week they become very sick at the stomach; they perspire profusely and vomit freely. After this they are, as they term it, seasoned to the employment. They become more healthy in their appearance, more florid in their complexion, and enjoy better health than any of the other inhabitants in the same locality.

While at Salina, I bled a sickly-looking sawyer, who was working in the marsh at some distance from the salt-works, and though he had not yet been attacked with fever, for that season, he had most of the premonitory symptoms. He felt unwell, his skin was sallow, the temperature of his body was only 95°, and the blood drawn from his vein had the peculiar appearance of that fluid when it is tainted with the marsh poison. I afterwards bled one of the men who was employed as a boiler of salt. That of the sawyer was dark in colour and evidently diseased. When the serum separated, it had a muddy brown colour, and an oily appearance, while the blood of the other was perfectly healthy; and though he was almost constantly in the swamp, the fluid which was drawn was even more florid than the blood of health, and the serum which separated was remarkably clear.

The fact of the exemption of the salt-boilers from the marsh fever was first, I believe, communicated to the public by Dr. Ludlow, in his 'Observations on the Lake Fevers of the Genesee Country.' I have also ascertained from personal inquiry, that those individuals who are employed in the numerous potteries, where the carbonate of potass is made from wood-ashes, are also remarkably exempt from fever, even when they are employed in the most swampy districts, but after a time, when they leave these works and are occupied in any other employment, they again become subject to the disease. It is also a fact, that the farmers, in some of the most sickly localities in the Genesee country, are remarkably exempt from the marsh fever; and this they attribute to the circumstance, that during the sickly season they live almost entirely on salt food, which they use on purpose to prevent the disease.

The electric or galvanic fluid blackens the blood out of the system, and we know, that when a powerful current of the electric fluid passes through the body, it destroys the saline matter of even the arterial blood, and makes it perfectly black *. When the febrile

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* It has been long known to the fishermen on the Banks of Newfoundland, that when lightning strikes near to a vessel,

poisons are taken into the circulation, they interfere with the agency of the saline matter, so as to darken the whole current; and I know many strong facts which go far to prove that when certain saline agents are used in excess, internally, they have a specific effect in preventing the action of these poisons on the living body. Several instances are already on record in the Western world, where persons have remained in the most sickly situations, and saved themselves from disease by small and frequent doses of certain salts, which they used regularly for the express purpose of preventing an attack of the prevailing fever.

Dr. Coventry, a respectable practitioner, now in the Lake country, and President of the Medical Society of the State of New York, states, in a paper, which is published in volume iv. of the 'New York Medical and Physical Journal,' that a wealthy planter, in South Carolina, had not left his habitation in the sickly season for many years, while his neighbours were obliged to emigrate annually during the hot months. The only precaution which he used was a small dose of sulphate of soda, taken in water, every morning, during the period that the fever was prevalent, and this enabled him to remain in safety, while his neighbours, who did not use the same precaution, were obliged to remove annually to some healthy locality. I know also an instance, which

the fish which are not yet salted become putrid almost immediately, while those that are salted do not suffer.

occurred in the West Indies, where almost every individual on board of a vessel was attacked with the African typhus; in fact, the only exceptions were two persons who took a small dose of Cheltenham salts every morning, as a preservative against the poison. It has also been confidently asserted, that of those persons in St. Petersburgh, who used the artificial saline waters as a preservative against cholera, few were attacked, and not one died.

I have formerly stated that the Indians of North America have long been aware that common salt is a perfect specific for the poison of the rattlesnake. When they are bitten by this reptile they immediately apply a ligature above the part, and scarify the wound with a sharp knife, which they carry for the purpose, and then stuff it with salt: when this is immediately done, the poison is destroyed. It does not affect the general system, and the part heals almost as fast as a common wound. I may state, also, that I have seen a rabbit that was under the influence of the rattlesnake poison, drink a saturated solution of muriate of soda with great avidity, and soon recover; while healthy rabbits would not taste one drop of the same strong saline water, when it was put before them. It is also well known, that when certain saline fluids are injected into the arteries after death, they prevent the effects of that poison which is sometimes so troublesome, and even dangerous to those who are frequently employed in the dissection of dead bodies.

A small quantity of salt water, mixed with a large body of fresh, will not prevent the whole from becoming putrid, or generating the marsh poison, when it becomes stagnant and exposed to the sun; but, when stagnant water is strongly impregnated with saline matter, it prevents the generation of the marsh poison. There is in the State of New Jersey, near to New York, several thousand acres of salt water marshes; yet the inhabitants who live near to these are exempt from the marsh fever. This has been stated by others, and I know it to be true from personal inquiry. It is also well known that though these persons are exempt from fever so long as the marshes are filled with salt water, yet in places where these have been partly drained, or where the salt water has been prevented from getting in, and when, after heavy rains, they are covered with fresh water, the intermitting fever has become epidemic: and this, in some places was so general, that the inhabitants were obliged, in order to prevent the fever, to break down the dykes, so as to allow the surface of the marsh to be covered by salt, in place of rain water.

The strong natural craving that all animals have for salt is universally known; but, in addition to this, almost every article of nourishment that we use contains either salts or the bases of saline matter. The saliva, the gastric and pancreatic juices, the bile, &c., all contain salts, similar to those which exist in the blood; and, probably, when required, a part of these is again returned to the circulation; for it has been ascertained, that even in animals that have been starved for days, the chyle contains a considerable portion of salts that were found *returning* into the circulation through the medium of the thoracic duct.

'The effects of salts upon the animal and vege-'table kingdoms are striking and important, and 'have furnished objects of the most interesting in-'quiry to the physiologist, the chemist, the physician, 'and the agriculturist; it appears to be a natural 'stimulant to the digestive organs; and that animals 'are instinctively led to immense distances in pur-'suit of it; for proof of this fact the reader is referred 'to "*Parkes on the Repeal of the Salt Laws*," and 'to an interesting work by my late lamented friend, 'Sir Thomas Bernard, entitled, "*Case of the Salt 'Duties, with Proofs and Illustrations*[*]."

'I have myself,' says Dr. Paris, 'witnessed the 'bad effects of unsalted fish; and in my examination 'before a Committee of the House of Commons 'in 1818 appointed for the purpose of inquiring into 'the laws respecting the salt duties, I stated the 'great injury which the poorer classes in many dis-'tricts sustained in their health from an inability to 'procure this essential article. Lord Somerville (in 'his address to the Board of Agriculture) gave an 'interesting account of the effects of a punishment 'which formerly existed in Holland. The ancient 'laws of the country ordained men to be kept on

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[*] Pharmacologia, by J. A. Paris, M.D., &c. &c. vol. ii.

'bread alone, UNMIXED WITH SALT, as the SE-'VEREST punishment that could be inflicted upon 'them in their moist climate; *the effect was horrible:* 'these wretched criminals are said to have been 'DEVOURED BY WORMS, engendered in their own 'stomachs.'

As the blood is the most complicated of all fluids, it is consequently the most prone to the putrefactive change. This is counteracted, however, by two causes, *viz.*, its vitality and saline ingredients, which mutually co-operate to the same effect in preserving the blood in its healthy condition; so that increase of the one may compensate for the diminution of the other. Thus, at Salina, the marsh poison tends to weaken the vital power; but the saline effluvia so copiously applied to the lungs and surface of the body counteract the morbid effects which otherwise would have been produced by the influence of the poison. But when it happens, as in the last stage of fever, that the vitality is diminished as well as the quantity of the antiseptic salts, there is then a risk of a fatal decomposition in the whole circulating current, and this result can only be effectually prevented by the copious introduction of saline matter into the system. In fact, it was from observing how little the solids are injured and the rapidity with which the black blood became putrid immediately after death, in those who died from the West India fevers, that I was first induced to adopt the saline treatment in the cure of those diseases; and the certainty with which the blackest blood could be reddened by the alkaline salts, led almost immediately afterwards to the discovery (if it be one) that the natural salts of the blood are, in reality, the true cause of its red colour, and that its blackness in the last stage was owing entirely to the great diminution of its own saline ingredients, which we all knew were constituents of the blood; but none, I believe, until very lately, even in this country, were aware of the peculiarly important purposes which these energetic agents exercise in the circulating system.

Dr. Bostock, who is, I believe, almost the only modern writer who has even alluded to this subject, observes, 'Since we find that a certain quantity of 'saline matter is constantly present in the blood, as 'well as in all the other albuminous fluids, we are 'naturally led to conclude that these salts perform 'some useful purpose in the animal economy, yet 'we are at a loss to say what this purpose can 'be.' And after alluding to the speculations of the older writers with respect to the uses of the salts, he states that their 'suppositions are all gratuitous *.'

Saline matter is so essential to the gastric juice, that without salt the stomach can no more digest the food, than blood without its saline impregnation

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* It is a curious fact, that the salts of the blood sometimes crystallise in the body. 'Sir Everard Home, in dissecting an 'aneurismal tumour, found a mass of crystals, which were 'analyzed by Mr. Faraday, and are stated to have been "salts 'usually met with in the blood."'

can stimulate the heart. It is also essential to secretion: in fever, for example, all the secretions cease, almost exactly in proportion as the blood loses its saline impregnation, but the action of the kidneys, &c., again recommence when an excess of saline matter is thrown into the circulation: and this is, perhaps, one chief reason why the non-purgative salts are so essentially necessary in that disease.

Dr. Prout has discovered that the quantity of muriate of soda is greatly diminished in the urine about twenty-four hours before death. It is probable, also, that about the same time, in most diseases, the quantity of saline matter is diminished in the blood.

I have stated some circumstances relative to the saline treatment in the West India fevers. I may also add that, formerly, during the hot months, the marsh fever was nearly as fatal in some parts of the Genesee country, as it is in Sierra Leone, or, perhaps, in any other part of the world. It is not so now; for about seven years ago a new method of treatment was adopted by some of the physicians at Rochester, which has been uncommonly successful. This was not the result of any particular theory. It had been found, from long experience, that the old treatment, with bark, was of little use in the malignant form of that fever, and that the mere purgative practice was even worse than none,—while calomel, in some cases, acted with all the virulence of a poison[*]. At last, however, some of the physicians came to the judicious conclusion, that if they did no good, at all events they should do no harm; and by this they found that they had gained something, for from that moment the mortality was considerably less than it had been before.

During this period, some of them had observed that soda and seidlitz powders were decidedly useful in that fever. This led them to inquire whether it was the acids, or the carbonates which produced the favourable result. The acids were tried and found to produce the most destructive effects. They then tried the alkaline carbonates, and almost every case that was treated with these recovered. From that period, they have not used either emetics, calomel, acids, bark, or even the sulphate of quinine. For even in the worst cases, they trust almost implicitly to large doses of the carbonate of soda, and when this is used in sufficient quantities it seldom fails to effect a cure. Where there is great excitement, they bleed in the beginning, and where the bowels are not sufficiently open, they either combine a small quantity of rhubarb with the carbonate, or use a seidlitz powder. Under this treatment, the most malignant cases, which were formerly so fatal, are found now to be quite tractable; and almost the only fatal cases which still occur, are in those unfortunate individuals, who fall into the hands of young practitioners, who, new fledged from the schools, and proud of their learning, brandish for a time the prescriptions of Rush, Chapman, Potter, &c., until their want of success, and the diminished number of their patients, compel them to adopt the more successful remedies used by the practitioners in the back woods, even in opposition to the famous dogmas taught *ex cathedrâ* by their most erudite Professors in the larger cities.

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[*] As there is, in these cases, an excess of muriatic acid in the stomach, it is probable that the submuriate is converted into sublimate. Dr. Prout thinks that this is not a very uncommon occurrence, in other cases, particularly in children, when there is an excess of muriatic acid in the gastric organs.

When at Rochester, in the month of September, 1830, I visited a number of cases of this fever, with Drs. Henry, Elwood, Colman, &c. Dr. Henry is now the oldest physician in Rochester, and decidedly one of the most able practitioners in the Genesee country, or, perhaps, in any other part of the western world. During these visits, observing that in every stage of the disease he prescribed the carbonate of soda in large doses, I asked him on what principle he used that medicine so much; his answer was, that he trusted to this, almost entirely, not from any theory, but merely because he had found, from long experience, that this was decidedly the best remedy for curing the disease. On the same day we visited a very malignant case of this fever, to which he had been called for the first time. The stomach was so excessively tender that the patient could scarcely allow even the slightest pressure to be made on the epigastrium. The tongue was exceedingly foul, and when I applied a small piece of moist litmus paper to this organ, the test was reddened almost as suddenly as if it had been dipped into a strong acid. The same thing occurred when litmus paper was dipped in the fluid ejected from the stomach. As test papers are seldom used in the back settlements of America, I dipped this reddened paper into a solution of the carbonate of soda, and showed him how completely the acid in the stomach must be destroyed by the medicine which he used; and, in fact, on the following morning, we found that soon after the patient had commenced the use of the carbonate, the burning and tenderness in the stomach had entirely ceased.

As there had been considerable excitement at our first visit, the patient was bled: one half of the diseased blood was received in one bowl, and this was allowed to rest without any addition. I added to the other half that was drawn a small quantity of carbonate of soda. When the former coagulated, it had a greater resemblance to oatmeal gruel, than to healthy blood. In place of being cupped, as in inflammation, it was *convex and quite irregular* on the upper surface, while the serum which separated had both a brownish colour and an oily appearance. The fibrin of the blood, to which the salt had been applied, coagulated; but there was no crust on the surface. The colour was florid, and the serum which separated was perfectly clear. Dr. Henry observed, that the first, to which no addition had been made, was a fair specimen of the diseased blood in the aggravated form of the lake fever, and could scarcely credit the evidence of his own senses, when he witnessed the remarkable effect, which even a small quantity of the carbonate had in preventing the diseased appearance in the blood. Dr. H. had been, for the last seven years, a most successful practitioner in fever; but, during that period, he, like many others, had not been aware of the reason why his remedies had been so successful.

About the year 1795, Dr. Mitchell of New York brought forward, as new, an old theory, according to which, fever is produced by an acid in the stomach; and alkalies, of course, or alkaline carbonates, were recommended as the proper means for effecting the cure. It is very evident, however, to those who have attended to this subject, that the acid which is so frequently met with in the gastric organs, is not the *cause*, but the *effect* of fever: for in most fevers, there is no acid in the stomach during the first stage; and though the carbonates, like other salts, may be useful before the attack, by neutralizing or preventing the action of the poison, yet their effects are most apparent in the middle and last stages of certain fevers, where there is almost invariably an excess of acid in the gastric organs. It is then that the alkaline carbonates are of such specific value; for they not only neutralize the acid, and relieve the gastric irritation, but they are instantly converted into fixed salts, which enter the circulation almost immediately, and act with great energy in remedying the vitiated state of the vital current.

Dr. Baker, of Portland, states, in a paper which was published many years ago in the 'Medical Repository,' that he had used the alkaline carbonates in fever, and that, under this treatment, he had scarcely lost a case during nearly three years' extensive practice. This, however, at the time, made no impression on the medical world: for it was published at a period when pure solidism was the doctrine of the day. The fact of the success was not doubted; but as the result was in direct opposition to all the prevailing theories, it was considered as accidental: and such is the withering effects of blind attachment to preconceived opinions, that the generality of practitioners preferred to be unsuccessful with emetics, mercury, opium, acids, brandy, &c., rather than adopt a practice, which had been proved, both by Dr. Baker and others, to be infinitely more successful than that which they were then using in the larger cities *.

I have formerly said that the vital principle opposes itself only to those noxious agents and che-

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* There are many physicians who prefer the maxim of Ariste, and consider it better to continue in error with the multitude, rather than to be the first to open their eyes to even the most positive evidence, so long as the fact is not generally admitted.

'Mais je tiens qu'il est mal, sur quoi que l'on se fonde,
'De fuir obstinément ce que suit tout le monde,
'Et qu'il vaut mieux souffrir d'être au nombre des fous
'Que du sage parti se voir seul contre tous.'

MOLIERE.

mical changes that are destructive to life; but the fact is, that it equally facilitates those changes that are conducive to health. When there is a deficiency of saline matter in the blood, as in the last stage of fever, and when certain salts are taken into the stomach, they are rapidly carried into the circulation, and almost instantly remedy the diseased properties of the vital current. By changing the condition of the blood, they correct the diseased qualities of the secretions. The urine, for example, which is passed in the last stage, is not only small in quantity but exceedingly acid; but the carbonates, either directly or indirectly, correct the acidity in this and the other secretions, almost as suddenly, and in the same way, as they effect these changes out of the body: while these, like the stronger salts, by their action as diuretics, assist the preservative power in removing the noxious agents from the living body.

It is well known that, in local inflammations, and in some other diseases, the blood which is drawn is so much deranged, that a white albuminous crust forms on the surface of the crassamentum. This is particularly the case in the aggravated form of the marsh fever. Now in this disease, when certain salts are used internally, they remedy the diseased state of the blood so completely, that the fluid which is drawn after they have been used for a short time is often entirely free from crust. I have stated that I have seen some cases of the marsh fever, where, when saline medicines had not been used, the blood which was drawn had, when it coagulated, exactly the appearance of oatmeal gruel. Yet we have seen that, out of the system, when a small portion of certain saline agents were added at a certain period, even to this diseased fluid, though the fibrin coagulated, still there was not even the slightest appearance of crust on the surface, or disease in the blood, whilst the serum which separated was perfectly clear. Now, may we not infer from this, that an extra proportion of saline matter, either in or out of the system, by increasing the saline strength of the serum, enables it to retain the albumen, &c., in a firmer state of solution? This prevents congestion in the solids, for, by rendering the current more fluid, it enables the blood to circulate more freely in the extreme vessels of the living body; it prevents also that morbid coagulation, or white crust, which is sure to occur, unless we prevent its formation by the internal use of certain salts; for when these are freely used internally, the blood which is drawn afterwards, is either without a crust, or, if it be crusted, it is much less so than in similar cases where these remedies have not been used.

As late as the year 1825, M. Guyon, a French physician, in Martinique, published a work for the express purpose of condemning the use of the lancet, purgatives, and blisters, in the treatment of the yellow fever. He recommended, in their place, bark, wine, and opium. These remedies were, I believe, confined almost entirely to his own practice, and the result was such, that I believe they will never be again used in the French islands. About the year 1823, M. Le Fort, and some of the other older practitioners in those islands, adopted the practice of bleeding freely, in the beginning of the disease, and then giving the quinine freely in the last stage. This practice has been attended with great success; and I have no doubt that the French physicians would have been still more successful, had they not combined this treatment with acids, opium, frictions with lime or lemon-juice, and the application of red-hot irons to the nape of the neck: as if such remedies could redden the black blood of fever, or remedy the vitiated state of the whole vital current. I observe that a similar treatment has lately been recommended in cholera; 'mais en médécine, il ne faut s'étonner de rien.'

The effect of the saline medicines and that of the acids are directly opposed to each other; consequently in fever, and all other diseases, where the one is decidedly beneficial, the other must be altogether inadmissible: for, if the saline treatment be the best, those agents must be the worst, whose effects are directly the reverse of the alkaline salts. When these are mixed with even the blackest blood, out of the body, they brighten its colour; whilst the acids not only completely deprive it of its redness, but they immediately destroy its healthy appearance, and when these are used internally in the last stage of fever, they have precisely the same effect: they ruin the structure of the already diseased blood, they add to the darkness of its colour, and rapidly bring on the black vomit, as well as the other malignant symptoms, by converting the whole of the vital stream into a vitiated fluid, which is totally incapable of supporting life.

The fact has long been admitted, that the acids reduce the force of the circulation. This has been accounted for in various ways; but my belief is, that they reduce the excitement of the whole system, and act as sedatives by lessening the stimulating power of the vital current*; consequently in local inflammation, in symptomatic fever, and all cases of common increased excitement, where there is no risk from dissolution of the blood, the acids are, perhaps, the best remedies that can be used.

But, however valuable the acids may be in all cases where we wish to reduce either general or local excitement, yet they ought never to be used, either in the malignant fevers or any other disease, where the blood is already but too black. In severe cases of such diseases, the saline medicines are valuable, beyond all calculation, while the acids are almost certain to cause death.

If it be admitted that a knowledge of the natural properties of the blood can enable us to lessen the mortality, in many diseases, we must allow also that incorrect theories on this subject have done much evil: for chemistry has thrown both its lights and its shadows on the science of medicine. The black and dissolved state of the blood, both in fever, scurvy, and other diseases, was so apparent as to draw the attention of practitioners to this subject, even at a time, when pure solidism was the reigning doctrine of the day: and from a belief that oxygen was the cause of the arterial colour, they adopted, in an evil hour, the idea of curing these diseases by means of acids, and other agents that contain oxygen in a separable form. It is not, however, by *addition* but *by subtraction* that oxygen reddens the blood either in the lungs or out of the body, and had those who adopted the theory of reddening the black blood in fever by means of acids, taken the precaution of trying the effects of these agents on the blood, out of the body, they would have found, that, so far from oxygenating or brightening the colour, they converted even arterial blood into a fluid, that required only the addition of a little water to make it resemble exactly the black vomit, which is, in reality, neither more nor less than an internal effusion of the black, thin, and dissolved blood which is thrown into the stomach and intestines, in consequence of the state of congestion in the *vena portæ*, caused, as I believe, by the total cessation of circulation in the liver.

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* It is a curious fact, that when strangers first arrive in the West Indies, they have a fondness for the vegetable acids, and a dislike to salted food, but after they become seasoned to the climate, the appetite is reversed, for they then have generally a fondness for salted provisions, and frequently a dislike even to the acid fruits.

As far back as the year 1793, Drs. Physic and Cathrall, of Philadelphia, in dissecting the bodies of some persons who had died of the African typhus, discovered the matter of black vomit in the arteries leading to the stomach. In a conversation which I had with Dr. Physic on this subject, he informed me that both Dr. Cathrall and himself believed, at the time, that this black fluid had found its way into those arteries, by some sort of regurgitation. Dr. Physic is the venerable patriarch of his profession in the United States, and not only a good, but decidedly a gifted man. He was one of the favourite pupils of Mr. John Hunter, and in his whole career has done honour to his great master. Dr. P. is as likely to draw a correct inference from a given fact as any individual that I know; but these dissections were made at a period of great mortality, when they had little time for minute investigation. If this had taken place, the black matter would have been traced to the heart, and, probably, long ere this, would have led to some important conclusions, both with respect to the theory and treatment of fever.

It would be impossible for even the most bigoted solidist to look at the black and vitiated blood which we see oozing from the eyes, the tongue, and the gums, in the last stage of the West Indian fevers, and not be convinced, when the vital fluid is in such a morbid condition, that this must have no mean share in producing the malignant symptoms, which are so common in those fatal diseases. Chisholm, however, I believe, was the first who adopted the idea of attempting to brighten, or, as he called it, to oxygenate the black blood in the West India fevers, by means of agents that contain oxygen in a state of loose combination. He commenced first with those preparations of mercury, which were supposed to contain oxygen in its most separable form; but we know well that this practice completely failed. He then thought of reddening the black and diseased blood by means of acids, &c. This idea was probably adopted at an earlier period; but it was only in the year 1798, that he had, for the first time, an opportunity of giving this practice a fair trial, in the African typhus.

In the month of March, 1798, he received 'a sufficient quantity of genuine and faithfully prepared 'nitrous acid,' from Dr. Rolla, Surgeon-general to the Royal Artillery at Woolwich. The first and only case in which this remedy was tried terminated fatally. '*The effects*,' says Chisholm, '*were dreadful; a most violent spasm of the stomach was instantly produced, although no irritability of that organ existed before;* and no application whatever 'afterwards could remove it. A repetition was consequently carefully avoided.'

Chisholm has published this case in the Appendix to his work; I will insert it here, for the benefit of those who may still believe that the black blood, in the African typhus, cholera, and other malignant diseases, may be reddened by acids and other agents that contain oxygen in a state of loose combination.

'Thomas Wilshaw, aged 29, and of a dark complexion, admitted May 9th, 1798. After slight

‘ rigour, was suddenly attacked last night with pain
‘ in the loins, and generally over the body; pain in
‘ the occiput, succeeded by an accession of heat, great
‘ thirst, &c. These symptoms have continued. Pulse
‘ 120; tongue whitish; belly open. Ordered the ni-
‘ trous acid in the following form: ℞ acid. nitros.
‘ ʒi, aq. lib. 1½, syrup. ℥iij. M. sumend. in die. In
‘ the evening *swote* considerably. 10th. Attacked,
‘ during the night, with violent looseness without
‘ griping; stools thin and yellowish: in the morning
‘ a vomiting came on. The nitrous acid in the fore-
‘ going dilution was continued in small doses, *so as*
‘ *to finish the bottle.* The vomiting, however, *in-*
‘ *creasing to an alarming degree,* the bottle was not re-
‘ newed, but various means were recurred to to absorb
‘ the acid already received. The vomiting could not
‘ be checked, and the discharged fluid was of a most
‘ acrid sourness. 11th. The same distressing symp-
‘ toms having augmented, the whole of the day was
‘ employed in the assiduous use of magnesia, with
‘ essence of peppermint, æther, opiates, &c., and
‘ blisters were applied: all these proved totally in-
‘ effectual. 12th. Pulse 68; tongue red, and dry in
‘ the centre, edges moist and white. The vomiting
‘ having somewhat abated, and it being evident that
‘ the nitrous acid could no longer be employed with-
‘ out hazarding the life of the patient, it was totally
‘ laid aside, and *calomel, with the occasional addition*
‘ *of the soluble mercury*[*], was substituted. 13th. Vo-
‘ miting somewhat troublesome; other symptoms as

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[*] Corrosive sublimate.

'before; no mercurial action. Continue. 14th. Appearance of yellowness in the eyes and face, former greatly inflamed. Return of vomiting to as violent a degree as on the 10th and 11th. Pulse 64, very feeble and tremulous. Subsultus tendinum. Teeth and gums covered with black fur; blisters applied to the back, stomach, and thighs. Six, P.M. Stomach became suddenly more retentive, but pulse more and more sunk; at ten P.M. expired. The examination of the body exhibited the fatal effects of the administration of nitrous acid in this tremendous fever. *The stomach was very much diminished in size, the villous coat everywhere abraded, and evident marks of gangrene.*

'In this single case, the nitrous acid was employed; but its effects were by no means such as to induce us to make, or to warrant a second trial. It is evidently inappropriate in a disease where the stomach is in a highly morbid state, and where, consequently, its use must necessarily produce an irritation, rendering that organ incapable of retention, or of performing its peculiar functions.'

Chisholm was the first, also, who used the chlorate of potass in the West India fevers. This, like the nitrous acid, &c., was given with the view of oxygenating* the black blood. In some cases it was found to be exceedingly useful; but in others it completely failed; for, from not being aware of the manner in which this most active and valuable remedy produces its effects, other improper agents were used at the same time, and the advantage that was gained by this salt was more than counterbalanced by the evil that was produced by large doses of calomel, corrosive sublimate, antimonial preparations, opium, and other agents that are so destructive, particularly when they are used in the last stage of malignant diseases*.

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* Chisholm was as much mistaken in supposing that the oxymuriate of potass contains oxygen in a separable form, as he was in believing that the addition of oxygen was the cause of the red colour of the blood. The chlorate and the nitrate of potass give out oxygen only when exposed to a red heat; but neither of

The deleterious effects of the acids in the West

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these salts can communicate one particle of oxygen to the blood at the common temperature of 98°.

* There is one very singular case, related by Chisholm, 'of 'enlarged spleen, and dropsical swelling of the abdomen with 'hydrocele, which shows the powerful effects which the chlorate 'produces in the secreting organs. In this the nitrous acid, in 'any state of dilution, *could not be made to sit on the stomach;* 'and therefore, the oxygenated muriate of potash was substi'tuted, in doses of four grains only, repeated every four hours. 'This medicine, thus exhibited, produced, on the third day, so 'remarkable a whiteness of the tongue, as to excite the astonish'ment of even the attendants; and within a week the secretion 'of urine was considerably increased, and the swelling of the 'abdomen almost entirely reduced. The Ordnance surgeon had 'it in view to effect a cure of the hydrocele, which was of a 'year's standing, by the injecting of diluted wine, but the 'oxygenated muriate, *by creating an absorption of the fluid, dis'appointed him, and completed the cure.'* Chisholm states, also, that the '*blood, after the exhibition of the oxygenated muriate, 'was remarkably florid.'* This was observed in two cases, wherein blood was drawn with a view to ascertain the change produced on it by the medicine.

India fevers, are now so well known to the well-informed practitioners in that part of the world, that we have only an opportunity of judging of their effects, when they are either advised by ignorance or used by mistake. The two following cases may not be without interest; but as they will probably be afterwards referred to, I shall now, from the fear of being tedious, describe them as briefly as I can.

Count Raven, a young officer in the Danish royal West India troops, was attacked with the climate or seasoning fever, in the island of St. Thomas, on the 17th of July, 1825. He was attended by an army physician, who had been but a short time in the West Indies. Mercury was used to a considerable extent. It was given internally, and two soldiers were also employed to rub him with mercurial ointment on various parts of the body. Salivation speedily came on, and as the state of his mouth was very disagreeable, he begged of his physician to allow him to wash his mouth with vinegar and water; this was permitted; but he was warned not to swallow any part of the fluid. A mulatto woman, however, who was allowed to be in the room, recommended him to swallow a quantity of the vinegar, which he did. This was on the 20th, and almost immediately after symptoms of a most malignant nature made their appearance; the stomach became exceedingly irritable, and in twenty-four hours, from the time that he had swallowed a considerable quantity of the acid, this fine young man was numbered with the dead.

I did not see the above case, but it made a considerable sensation in St. Thomas at the time that it occurred. The above facts were given to me in writing by a gentleman who was present during the short illness of this young officer.

In the year 1828, a party of strolling comedians, from the United States of America, visited the island of St. Thomas. There were fifteen in all, and during their short residence, eight were attacked with fever. All of them were attended either by Dr. Stedman, or myself: one died and seven recovered. The following is an abridgment of the notes which were taken of the unsuccessful case, at the time that it occurred.

Joseph Kenyon, about thirty-three years of age, rather stout, with a florid complexion, is a native of England, but has resided several years in the United States. Arrived in St. Thomas on the 28th October. This is his first visit to any of the West India islands.

The theatre in St. Thomas is uncommonly hot, and this man, like the other actors, perspired most profusely while he was performing on the stage. He escaped the climate fever, however, until the middle of December. On the 16th of this month Kenyon passed the day on a visit to an old acquaintance, who was then captain of a vessel that was lying in the harbour. The wind was southerly at the time, and the day was uncommonly hot for that season of the year. They had been sailing the greater part of the forenoon, about the harbour, in an open boat. In the evening he felt heated and unwell, but drank even more than usual *, on purpose to get rid of his bad feelings. A violent fever came on during the night, but they thought lightly of this, as it was believed to be partly the effect of the evening's debauch. The fever, however, continued during the day, but he refused to send for any medical attendant, and said that he would try to sleep it off, consequently the whole of that day was lost. On the morning of the 18th I was sent for to see him. His fever had commenced without any chill; he was suffering from severe pain, particularly in the head; his eyes were red, and his face exceedingly flushed, but his pulse was only 108; the artery at the wrist, however, was completely incompressible, and felt almost as large as the little finger. About forty ounces of blood were immediately taken from the arm; it was hot and florid, as it always is in this fever; but the crassamentum was not cupped, neither had it the slightest appearance of crust on the surface. The bleeding produced instant relief. About half an hour after this he took a croton pill: which was retained, and operated freely; the morbid heat was also removed from the system by the almost constant use of a sponge, frequently dipped in cold water.

As the excitement continued, he was again twice

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* One of his companions, using the language of Shakspeare, told me, that 'Kenyon had been long in the habit of applying 'hot and rebellious liquors to his blood;' and that all of them feared, even from the first, that he would fall a victim to fever in the West Indies.

bled during the day, and on the following morning, when I found that we had now nothing to fear from increased action, he was then put under the saline treatment. On the 20th, he was much better, the fever still continued, but he was free from headache, and without any one of the malignant symptoms.

When I saw him late on the night of the 20th, he was then considerably better. His eyes had been slightly yellow for the last twenty-four hours, but the yellow colour had not increased; his kidneys were operating with considerable activity; he was cheerful, and appeared to be so well that I thought he was completely out of danger, and left him without any intention of returning on the following day.

About six o'clock, however, on the morning of the 21st, I was again sent for to visit this patient, and was told that he was dying. This was an unexpected blow to me, for, previous to that period, I had been so successful with the saline treatment, that I did not, at the moment, know what to think of this circumstance. When I went to the house, I met the servant coming out with a large basin nearly full of the black vomit, which, she told me, he had just brought up. When I entered the room he was still perfectly in his senses. His first words were, that his fate was fixed, for he had got the black vomit. His eyes and skin had become completely yellow within a few hours; he complained of a severe burning in the stomach, and begged me, most urgently, to give him something to relieve it as speedily as possible.

There was a box of Seidlitz powders on the table, which he had been using before, and, being the nearest remedy, I took the alkaline powder, and threw it into a tumbler which contained, as I supposed, about a few tea-spoonsful of water, but it effervesced immediately so briskly with the carbonates, that I was induced to ascertain what it was. In answer to my enquiries, he told me that, as soon as I had left him on the preceding evening, the landlady of the house had come up with a large tureen full of warm vinegar and water. This said lady is an old French woman, who has long kept a lodging-house in St. Thomas'; she told him that the English practitioners might be very clever, but they neglected many remedies that were always used by the best physicians in the French islands. She insisted upon bathing him with warm vinegar and water: to this he consented; it was done, and soon after he went to sleep. When she left him, he was apparently so little ill, that she did not leave either a servant to remain with him during the night, or a candle in the room. He awoke, however, about midnight with a burning at the stomach, and intense thirst; the guglet, however, had been taken away to be filled with water, and the servant had neglected to bring it back. He got up, as he said, half asleep and half awake, and as he could not find any water to quench his thirst, he dipped a tumbler into the tureen, and drank freely of the vinegar and water, which the old woman had, unfortunately, left in the room; but the thirst soon became more excessive than before, and in a short time it was so urgent that he could not sleep: he continued, however, from time to time drinking the vinegar and water; and, before day dawned, he had used almost the whole. The thirst had not been alleviated by the acid drink; on the contrary, it had produced a most intolerable burning in the epigastric region. About sunrise he became excessively sick at his stomach, and even the first time that he vomited, he brought up a large quantity of the black vomit. This was only a short period before I saw him, and then he felt relieved for the moment, from having got rid of so much of this black and acid irritating fluid from the gastric organs.

I gave him one of the alkaline powders. A sinapism was immediately applied to the region of the stomach, and afterwards carbonate of soda was administered in large doses, but the vomiting continued. When he became worse, creole pepper, nitrate of silver, champagne, and other remedies were then used, but without the slightest relief; for the stomach was so exceedingly irritable that all these agents were immediately rejected. At the request of one of his companions, mercurial ointment was rubbed in every half hour; but it seemed only to annoy him without producing any benefit. The kidneys had now ceased to secrete, and all the malignant symptoms were increasing with great rapidity. The vomiting became less frequent; but in proportion as this left him he became delirious, with frequent starting in the tendons. The increased excitement was nearly gone, his pulse was down to 76°, and his skin was rather colder than natural. There were moments, during the day, when he appeared to rally; but at night he became much worse. Early on the following morning he became convulsed, and after some awful struggles he expired, as I believe, a victim to the accidental use of the large quantity of the acid.

We need not, however, have recourse to solitary cases, for the purpose of proving the destructive effects of the acid treatment in the malignant fevers of hot climates; for in some countries this practice has been tried on a large scale: and we know well, that the mortality has been dreadful in every instance where acids have been extensively used in malignant diseases. The following is one of the many proofs which might be brought forward on this subject.

The African typhus made its appearance for the first time in the city of Carthagena, Old Spain, in the month of September, 1804, and ceased in January, 1805.

When the disease commenced, there were about thirty-four thousand inhabitants in the city. We are not informed of the exact number that were attacked during these months; but as some of the inhabitants fled from the disease, and as numbers in all towns are not susceptible of being acted on by contagious poisons, it is perhaps no extravagant arithmetic to suppose that, out of the whole, about ten thousand escaped any formal attack of this fever; consequently we may infer, that about twenty-four thousand were taken ill with the reigning epidemic.

During this period, the oxygenated muriatic acid gas was used as a preventive or preservative against the disease, but it completely failed. '*I was constantly,*' says Riseuno, '*surrounded with an atmosphere of this gas, whilst in the Military Hospital; the same was the case with several families, who, like myself, took the disease*[*].'

The Spanish physicians have a great dislike to the use of the lancet; they did not use calomel, but bark was with some of them a favourite remedy; for even during the hot stage they forced large doses of this substance, *combined with acids*, into the stomach, at a time when it was so exceedingly irritable, that it could scarcely retain one particle of this most nauseous mixture.

'*But,*' says Riseuno, '*the most rational method which has hitherto been pursued,* is that which has been recommended by those learned persons who have written on the subject, and it coincides with the opinion of the most enlightened professors of this city, and is confirmed by daily experience[†].'

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[*] See Riseuno's Paper in Sir William Burnett's Work on the Mediterranean Fever, p. 241.

[†] There is scarcely any other city where, in propoltion to the number attacked, the mortality has been so great as in Cartha-

This rational method consisted in giving tamarinds, or cream of tartar, in the beginning, in such doses as to act upon the bowels. Emetics were also administered; but their chief reliance was on acids, 'particularly those of the mineral kind;' and when the malignant symptoms made their appearance, which indeed they were sure to do under such practice, recourse was then had to the *concentrated mineral acids in large doses.*

It appears, however, that the ultimate result of this most rational treatment had not made any very favourable impression on the mind of Riseuno; for, after having seen the effects of their treatment in no less than four epidemics of this disease, he seems to have made up his mind, in case of its return, to leave the patients almost entirely to themselves; or, at all events, not to have recourse to acids or any other active treatment. The reason which he assigns for having adopted this resolution is not uninstructive.

In his letter to Sir William Burnett, he states, that 'It would afford me the greatest pleasure to be able 'to point out any method of treatment which would 'prove an alleviation to suffering humanity, under 'the heavy scourge which this city has been unfor- 'tunately subject to; but, my dear friend, I am 'reluctantly compelled to acknowledge, that during 'the four epidemics which have prevailed, I have not

gena, not only in one, but in all their epidemics. Now, as this is admitted, I cannot see on what principle Riseuno can assert that the theory was confirmed by the result of daily experience.

'been able to fix on any plan upon which I can invariably depend, in the treatment of this disease: 'a disease which, from the very first moment of attack, 'is of the most malignant nature of any hitherto 'known; for which reason, it ought to be combated 'on its very first appearance; *for, mild and simple as 'it may appear, it is of a most insidious nature, often 'appearing with symptoms which, if incautiously 'treated, lead to many others of a far worse description;* and whilst, under this idea, the effect of the 'medicine is awaited, before a more correct judgment 'of its efficacy can be formed, symptoms make their 'appearance which speedily deprive the patient of 'life; whilst, perhaps, no remedy has been applied, 'or, as frequently happens, if administered, it could 'not be retained on the stomach of the patient.

'From a consideration of the foregoing circumstances, *I am induced to prefer a passive method of 'treatment;* by which, I do not mean to insinuate 'that the physician should be altogether neutral, but 'that a general plan should be kept in view; whilst 'each individual ought to be treated in a manner the 'most appropriate to his constitution, temperament, 'age, manner of living, &c. &c., and under this invariable idea, *that the mildest, simplest, and easiest 'method, is that which is preferred by nature;* particularly in the complaint under consideration. This 'has been repeatedly observed, either in the case of 'those persons who, *being abandoned by their family 'and physician, are unexpectedly preserved;* whilst, 'on the other hand, many of those who were assisted 'by physicians, and who took and retained a great 'quantity of medicines, *expired under this treatment, 'and that of the violent fever.*'

Now, from this it appears that the only individuals who were 'most unexpectedly preserved,' were those poor persons who had the good fortune to be left to themselves; whilst the rich, who were attended by the Spanish physicians, and took plenty of emetics, bark, mineral acids, &c., were, as might be expected, sure to die, either from the fever, or from the effect of the remedies that were given to cure it*: and this is the more probable, from the unquestionable fact, that during the short period of four months, the population in Carthagena was reduced from thirty-four to fourteen thousand; for of the twenty-four thousand who were probably attacked, *twenty thousand fell victims*, either to the disease or the improper treatment which was, almost universally, adopted by the physicians of Spain. And my own conviction is, that if five thousand of these died of the fever, fifteen thousand fell victims to the improper treatment which had been adopted in Carthagena.

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* It was not without foundation that Le Sage was so sevère in his criticisms on the Spanish practice; neither was he far from the truth, where he makes Gil Blas say, when he thought himself at the point of death, in the Tower of Segovia,—'Je 'm'attendais donc à passer le pas; néanmoins mon attente fut 'trompée. Mes docteurs m'ayant abandonné, et laissé le champ 'libre à la nature, me sauvèrent par ce moyen. La fièvre, qui, 'selon leur pronostic, devait m'emporter, me quitta comme pour 'leur en donner le démenti.'

Such facts as these require no comment. There can be no question, that the practitioners in Spain acted according to the best of their knowledge; and though we know now that they were led into this destructive practice by a false theory, yet to have persevered, so obstinately, with agents that were so evidently destructive was, in my humble opinion, neither more nor less than the perpetration of legal murder on a large scale. I may observe also, that it is the miserable treatment that has been founded on erroneous opinions with respect to the blood, which has brought the humoral pathology into so much disrepute. The solidists, however, have little reason to refer to this, or to boast of the success of their own practice in the malignant diseases of hot climates; for were it necessary, I could bring forward many instances where the English physicians lost, in the same fever, a great deal more than one-half of those patients who had the misfortune to be treated with large doses of calomel, opium, and the other improper remedies which are generally used by those who attend only to the nerves, or to increased action in the brain or the mucous membranes.

It was early in the year 1827 that the African typhus made its re-appearance in St. Thomas, soon after the arrival of certain vessels, which came indirectly from the coast of Africa. As this disease has been repeatedly in that island, the cases which occurred were not numerous, and chiefly confined to strangers, but particularly to the sailors. It was not, however, entirely confined to these, for some of the creoles were severely attacked, and more than one of the natives, who had never been out of the tropics, died during this epidemic, on the third or fourth day after attack, with the yellow skin, the black vomit, and all the other symptoms of this virulent disease.

During this epidemic I lost some of my best friends. In these cases, I had administered the remedies in common use, not from a belief that they did any good, but merely because it was necessary to do something. In some instances, however, their effects were so obviously injurious that they could not be overlooked. It was during this period that I examined some of the bodies very minutely soon after death. The appearances which I observed, induced me to form a different opinion with respect to the nature of fever, and this led to the saline treatment. The first case in which it was tried, was in that of his Excellency, Chamberlain Von Scholten, Governor-General of the Danish West-India islands. I had seen this case in the very beginning of the fever. The patient was bled freely in the first stage, the cold sponge was almost constantly used, and after the bowels had been well evacuated with castor oil, &c., we continued to give purgatives in small doses, and along with this we used, also, an infusion of snake-root tea. In this case, the excitement had been sufficiently reduced by the free use of the lancet. There were no signs of organic disease in any of the solids. The disease, however, continued to advance rapidly in its progress, as I believed, in consequence of the vitiated state of the blood. The excitement at this period was not great; but on the third night of the fever he became exceedingly restless. The kidneys had ceased to secrete, and when the day dawned on the following morning, it was observed that, during the night, the eyes had become yellow. The skin was also assuming the same colour, particularly about the neck and the angles of the jaw.

I was now perfectly convinced, if I continued with the same treatment, that this active and valuable individual would certainly be lost. I had formerly received great kindness from this nobleman, whom I now considered to be in great danger. In fact, I had more reasons than one for feeling a deep interest in this case. It was now that I considered myself perfectly warranted in giving him a chance, by trying the effects of the non-purgative saline medicines. These were immediately used; in twenty-four hours afterwards he was out of danger, and is still, I trust, in good health.

This case gave me great encouragement to go on with the saline practice in others. I had afterwards several severe cases on shore, and many more amongst the shipping. At one period, I had about thirty-five sailors at the same time in the fever hospital. The disease continued chiefly, however, in solitary cases, until about the month of June, 1828.

Dr. George William Stedman adopted the same treatment, and I can safely assert, that after we commenced it, neither Dr. Stedman nor myself lost one case, where we were called (which we generally were) to the patients within the first twenty-four hours after the attack. Those, however, were not quite so fortunate, who obstinately persevered with the old practice, and continued fretting the stomachs of their patients with calomel and James's powder, or rubbing them all over with mercurial ointment. One physician, who lived in the same street, and nearly opposite, during the same epidemic, lost, in some instances, three out of four amongst the sailors; and, during the same period, his partner, his apothecary, and bookkeeper, were all attacked with a fever that commenced with a chill; and though he saw every one of these cases, early in the disease, yet they all died on the third, fourth or fifth day from the period of attack, with the symptoms of the African typhus distinctly marked. In short, during this epidemic, almost every case terminated in death which was treated, either by myself or others, with calomel, antimonial preparations, frictions with mercurial ointment, snake-root tea, &c.; whilst those that were well bled and properly evacuated in the beginning, and then put under the saline treatment, almost invariably recovered. These circumstances are well known in the island of St. Thomas, and I do not hesitate to assert, that on an average of the fever cases that were treated in this way, either by Dr.

Stedman or myself, forty-nine patients out of fifty recovered.

Should I, hereafter, be able to publish my intended work on the fevers of the West Indies, I may then bring forward additional evidence on this subject; but I trust that I have already said quite enough to convince any impartial reader, that nothing has been gained by the exclusive doctrine of the solidists, and that much has been lost, either by the neglect, or by the erroneous opinions which exist, even to this day, with respect to the healthy and diseased properties of the blood; but those who will contrast the effects of the treatment generally used in malignant diseases, with the result of the practice which is founded on the belief that the vitiated condition of the blood ought to be attended to in the treatment of fever, as well as the solids, may admit that the saline practice not only supports, but illustrates, the very doctrine which I have given of fever; whilst the theory, on the other hand, enables us to explain why the result should be so satisfactory, particularly in the treatment of all malignant diseases.

I have now given a hasty outline of the views which I have formed with respect to the nature of fever, and also of the treatment which I have adopted; and though I know well that it will never be possible to cure every malignant case, either by this or by any other practice, still my conviction is, that the saline treatment is decidedly the most successful that has yet been tried; for, under the use of this, a greater number has been saved than by any other. This practice, however, is as yet in its infancy; still, from what I have seen of its effects, not only in the West Indies, but even in this country, I believe that the day is not very distant, when the physician will no more think of curing a malignant disease without the use of the active non-purgative saline medicines, than he does, at present, of treating acute inflammation in the chest, without the aid of the lancet, or of destroying the syphilitic poison in the system, without the use of its antidote—mercury. Such, I may add, is not only my own conviction, but the same opinion has been expressed by others who have seen the saline treatment fairly and extensively put to the test, particularly in the treatment of Cholera in this metropolis.

[Soon after my return to London, in November, 1829, a paper, of which the following is a copy, was sent to the College of Physicians. It was not read however, until the 3d of May, 1830.]

OBSERVATIONS ON THE BLOOD,

BY WILLIAM STEVENS, M.D.

'In the study of anatomy, every man proceeds on the maxim, that nothing in the body of an animal was made in vain, and when he meets with a part of which the use is not obvious, he feels himself dissatisfied till he discovers some, at least, of the purposes to which it is subservient.'—Stewart's *Outlines of Moral Philosophy*.

'There is sometimes met with, in the West Indies, a malignant form of the yellow fever*, in which, from the beginning to the end, it is very evident from the symptoms that there is little or no affection of the solids, during life; and often after death even the most able anatomists cannot detect any trace of organic disease, either in the brain, the stomach, the intestines, or in fact in any of those organs, whose derangements are generally supposed to be the cause of fever. In those fatal cases, there is no excitement in the commencement sufficient to injure the solids, and we can only ascertain the real cause of death, when we open the heart, and examine the state of the once vital fluid. The cause of death then becomes evident; for, in place of blood, we find there a dissolved fluid, nearly as thin as water, almost as black as ink, and evidently so diseased, as to have been totally incapable of either stimulating the heart or supporting life. In both cavities of the heart the diseased blood was equally black, and in the whole vascular system, all distinction between arterial and venous was entirely lost. A close attention to some of these cases, first led me to believe that the influence of the nervous system was greatly over-rated in fever, and that the blood was infinitely more concerned as a cause, and the solids less than is generally believed. This and some other circumstances induced me to pay particular attention to this subject; in consequence of which I have made a

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* The term *yellow fever* was used in the above paper, in compliance with common usage. My conviction is, however, that this name has been the cause of much confusion, and ought to be entirely abandoned. Symptoms are effects, and certainly it is better to name diseases with a reference to their remote cause, than to designate them from effects which are not invariably

present, nor exclusively confined to them. The yellow colour of the skin is a common symptom, both in the climate fever and the African typhus. It is also occasionally met with in the marsh fever; but it does not always occur in any one of these diseases; and were we even to admit that they are mere varieties of the same fever, it is certainly improper to name it from a symptom which is not essential to the disease.

number of experiments on the blood, and these have led me to form certain opinions with respect to this fluid, some of which I now submit to the Royal College of Physicians, with that diffidence which every one must feel, who is fully aware how much all human reasoning is liable to error, particularly where the subject is so mysterious, and as yet so little understood.

'It has been the fashion for nearly a century past to overlook almost entirely the importance of the blood, as well as those derangements which take place in the vital fluid, either as a cause of morbid action in the solids, or as a consequence of disease. Yet, to the physician, a knowledge of these changes is valuable beyond all calculation. The effects of these derangements in producing disease, the changes that are produced in the circulating current by active agents, the manner in which these act, either as a cause of or as remedies in disease, form by far the most important part of the study of medicine. My reason for the belief *that fever is produced by a deranged state of the blood, and that death in bad fever is often caused by its dissolution*, will afterwards be given; but for the moment I may be allowed to say, that this long and entire neglect of the *vital fluid* is, perhaps, the chief reason why we have been going back in the theory of fever, and that, too, at a time when such brilliant improvements have been made in almost every other department of medical science. But be this as it may, perhaps the t
value of a healthy state of the blood will be

proved by the study of its derangements, and the effects which these derangements produce on the solids of the system.

'On examining, soon after death, the black and dissolved blood that had been taken from the heart of those who had died of the yellow fever, it was very evident, even at first sight, that several great changes had taken place.

'1st. The blood was more fluid than natural, partly from an excess of serum, probably produced by a stoppage of the secretions and a retention of those fluids in the system, which ought to have been thrown out by the secreting organs; but, independently of this, in these violent continued fevers, as little nourishment is used, the fibrin, or solid part, does not appear to be formed in its usual quantity, and perhaps, also, it is exhausted faster than in health, in supporting the high and continued excessive heat that is so great in the commencement of the inflammatory form of the West India fevers. It may be proper to state also, that in the first stage of the climate, or endemic yellow fever, the structure of the red globules is frequently deranged; which is evident from the fact, that in this fever the colouring matter is often detached from the globules, and dissolved in the serum, giving to that part of the blood, when it separates from the fibrin, a bright scarlet colour; and this colouring matter is so completely dissolved in the serum that it cannot be separated, either by filtration or any other mechanical means.

'2nd. In proportion as the disease advances, the colour of the whole mass of blood, both in the arteries and veins, changes from its natural scarlet, or Modena red, to a dark black. I have frequently filled one glass with the black fluid, which was taken from the heart soon after death, and another with the black vomit taken from the stomach. They were both so unlike the blood of health, and resembled each other so completely, that it was almost impossible to distinguish the one from the other; and from its appearance it was very evident, that such diseased blood could no more stimulate the heart, or support life in the solids, than putrid water can nourish vegetables, or carbonic acid gas support respiration.

'3rd. In violent continued fevers, the saline matter, like the fibrin, &c., appears to be exhausted faster than it enters the circulation; for the blood soon loses a great proportion of its saline impregnation, it consequently loses entirely its saline taste, and we shall afterwards see *that the black colour of the blood is a certain proof of the entire loss, or, at least, of the great diminution of the saline ingredients.*

'4th. The blood, though dissolved, was not yet putrid; for the blood is so essential to life, that putrefaction of this fluid cannot exist in a living body. But dissolution is, probably, the first step towards the putrefactive process; and when this, to a certain degree, takes place, death of all the solids must inevitably follow. I may here add, that this *dissolved* state of the vital fluid was the *cause,* and not the effect of death; for I have sometimes seen the blood even *previously to death*, both black and so thin, that it could scarcely be retained in its own vessels; and occasionally it has been observed oozing from the tongue, the eyes, the skin, and other surfaces, where there was not even the slightest lesion in the solids.

‘ *The dissolved state of the blood is the effect, and not the cause, of fever.* But as it was evident in many of the fatal cases, that this dissolution *was the sole cause of death,* it then became an object of importance to find out some agent capable of preventing this fatal change in the whole circulating current.

‘ In all climates nature has given to the waters of the deep the power of self-preservation, and this power they probably owe partly to their saline impregnation. It is also well known, that some of the saline agents possess great antiseptic power; and those who wish to preserve either the animal solids or fluids after death, find that certain saline substances are by far the most effective agents for this purpose. A given proportion of saline ingredients is also invariably found in healthy blood, and the presence of these seems to be essentially necessary to its healthy state; for, when the salts are lost in disease, the vital fluid goes fast to decay. Now, as this saline matter was lost, or greatly diminished in the blood that had been dissolved in bad fever, and as the loss of its antiseptic salts was probably the chief cause of the mortality, I was induced to try the effects of certain saline remedies in preventing the bad symptoms that are so generally met with in the fevers of hot climates; and after having used several of the saline medicines in a number of cases, I was completely convinced that those agents had, when used at a proper period of the disease, a specific effect in preventing those changes in the blood, which, as I believed, were the chief cause of death. And the fact is, that in all the cases in which they were timely and properly administered, they prevented the bad fœtor in the breath, the stoppage of the secretions, the yellow colour in the skin, the black vomit, and the other fatal symptoms which were so common in similar cases, where these medicines were not used.

'After having often witnessed the specific effects of the saline medicines in preventing the mortality in malignant fevers, and recollecting that several of the neutral salts enter the circulation without undergoing any decomposition in transitu, I was induced to try what effects these agents would have when mixed directly with the blood, just drawn from the system, while it was still warm and fluid. I was in some degree prepared for the result of these experiments, from having so often seen the powerful effects of these medicines in remedying the diseased properties of the blood, even in the most aggravated forms of malignant fevers.

'It is well known that the saline principle is the new agent, which is formed by the saturation of an acid with either an alkali or an earth. An immense variety of salts are produced in this way, but they all possess certain properties in common. Some of them are plentifully formed by nature, others by art; but, however formed, they produce the most striking and important effects, both in animal and vegetable life; they possess new properties totally separate and distinct from any of the substances from which they were originally formed; and one of these properties is the power *which every one of them possesses of giving a rich arterial colour to venous blood*. This property is common in them all, and the degree to which they possess it is, perhaps, the best test of their purity as saline agents.

Whatever that animal substance may be, which gives to the blood its colour, it is evident that these agents, which change the colour of this fluid, must do so by acting on the colouring matter. To ascertain the effects of different agents on the blood, I made a number of experiments, and in these it was observed:—

1st, That *all* acids give a dark colour to the colouring matter of healthy blood; and in proportion to their strength, they change the colour from red to black, as certainly as they change vegetable colours from blue to red. When any one of the acids was diffused in a small quantity of water, and then mixed with the fluid blood, the colour of the whole was immediately changed from red to black. Even the vegetable acids so completely blackened the blood, that the addition of a little water converted the whole into a fluid exactly resembling the black vomit.

'2nd. That the *pure alkalies* have a similar effect with the acids, in changing the colour of the blood from red to black, though not in the same degree.

'3rd. *That the whole of the neutral alkaline salts* immediately changed the venous blood from a dark Modena red to a bright arterial colour.

'4th. That even those salts that contain an excess of alkali, the subcarbonate of soda for example, immediately changed the venous blood to a beautiful bright arterial colour*.

'5th. *That when the neutral salts were mixed with the dark and dissolved blood that had been taken from the heart of those who died from the yellow fever, even this black and dissolved fluid was immediately changed from black to a colour that was highly arterial.*

'Nature does nothing in vain; and all the analyses of the blood have proved, that in health it invariably contains, in every part of the world, a given proportion of saline matter. This is not accidental, for it is essential to the healthy properties of the current, and exists as invariably in healthy blood as the water, the fibrin, the albumen, or the colouring matter. Arterial blood must evidently contain a larger proportion of this saline matter than venous; for all the solids, and most of the secretions, derive their saline matter from arterial blood. But the serum of even the venous blood which is left, contains a proportion of about thirteen ounces to the thousand of these salts, independently of what is lost by evaporation, &c. We all know how active these salts are as chemical agents; and these saline ingredients are so constantly found in the blood, and in healthy blood their proportion is so exact, that we are forced to believe they are placed there for some important use; and that they do serve more important purposes than one is very evident. But the importance of this saline impregnation has been almost entirely overlooked, from the great attention that has been paid, in all ages, both by physicians and philosophers, to the much less important colouring matter.

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* The effects of these experiments are best seen when they are made on *healthy* blood; the agents ought first to be dissolved in a little soft water, and then well mixed with the warm blood, before it begins to coagulate.

The nature of this paper prevents me from entering minutely on the important effects which the saline ingredients produce in the vital fluid; but, in a work which will soon be published, I shall endeavour to prove, first, *that the blood owes its red colour to this saline impregnation. Black appears to be the natural colour of the colouring matter; for when we take a clot of blood, and deprive it completely of its saline matter, by immersing it in fresh water, the colouring matter soon becomes so black, that even oxygen has no effect in changing its colour. But when we immerse this black clot in an artificial serum, made by dissolving some saline matter in water, the black clot in this clear fluid assumes almost immediately a beautiful bright arte-*

rial colour. Secondly, that to this saline matter, the fibrin, &c., owe their fluidity, for the fibrin remains fluid *only while dissolved in the saline fluid, and becomes solid when the saline matter leaves it to unite more closely with the water to form the serum.* Thirdly, *that* the alteration of form which this saline matter undergoes, when the blood is converted from arterial to venous, and from venous to arterial, changes its capacity for caloric, and gives it an influence in supporting the temperature of the system. *The saline impregnation also adds to the stimulating quality of the blood, and assists, even in a high temperature, in adding to its power of self-preservation.*

'In their present state, both medicine and its dependent branch, animal chemistry, are, as yet, far from perfection, for in these the doctrines of yesterday are but too often overturned by the doctrines of to-day; and so long as theories are constantly changing, we may, perhaps, best gain our end by simply resting on facts; and the facts that I shall endeavour to establish are partly these:—

'1st. That in violent, continued, or malignant fevers, even where proper means are used to protect the organs by reducing the excitement, chemical changes often taken place in the whole circulating current; and in such fevers these changes are frequently the sole cause of the mortality. In proportion as the disease advances the blood loses its solid part, and becomes thin; it loses its saline matter, and becomes both black and vapid; it loses servative power, and goes fast to decay; in consequence of which it loses its vitality, and in a short period becomes totally incapable of either stimulating the heart, or supporting life. The degree to which these changes take place is in proportion to the malignancy of the disease. In the yellow fever, in the African typhus*, in the plague, &c., dissolution or diseased changes in the blood is a common cause of death. The typhus of cold countries is, comparatively speaking, a mild disease; but even in the common typhus similar changes take place in the blood, though in a less degree, as has been proved by the important experiments of Dr. Reid Clanny, of Sunderland.

'2nd. In all varieties of malignant fever †, the loss of the saline or preservative power appears to be, in every instance, the chief cause of the diseased condition, or entire dissolution, of the blood.

'3rd. Where proper means are used to protect the organs from the increased excitement during the early stage of the disease, and after the excitement is sufficiently reduced, when saline medicines are timely and judiciously used, and proper nourishment is given, the bad symptoms are generally prevented. When saline medicines are administered they do not fret the stomach, they act on the intestines as much as is necessary, they keep up all the secretions, but particularly that of the kidneys, and enough is absorbed to enter the circulation, and prevent the dissolution of the blood, and preserve it until the fever abates, and the danger is past. This I am warranted to state as a fact, inasmuch as this treatment was commenced in the West Indies, in the year 1827[*]; and since then it has stood the test in several hundred cases of the West India fevers, where it has been tried both by myself and others, and with scarcely a single loss when we were called to the patients within the first twenty-four hours after the attack, and with very few deaths, where we were called in previously to the commencement of the fatal symptoms. My friend, Dr. George William Stedman, now of St. Thomas, and others, have adopted the same treatment, and the result in their cases has been similar to that which occurred in my own practice.

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* Or the Bulam fever of Chisholm.

† In referring to the definition which I have given of fever, in a former part of this volume, it will be seen that the plague and cholera are included in the list of those that are produced by the aërial poisons. The same definition applies also to those malignant cases of the African typhus, in which there is a want of action from first to last. In cholera the blood is evidently diseased before the attack, and it is also worthy of remark, that in all those cases of that disease which I have seen recover under the saline treatment, there has scarcely been a single instance where the relapse was followed by a hot stage.

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[*] Dr. Clanny's lecture on Typhus fever was not published until 1828; but independent of this, Dr. Clanny's practice and that which I have recommended, are in direct opposition to each other. I may add, also, that Dr. C. disavows all connexion with the humoral pathology, and considers the diseased changes in the blood as merely the effect of debility in the solids. The only ingredient which is considered by him as of any essential value, in the vital current, is the carbonic acid, and this he believes to be 'to the blood what the spiral spring is to the balance w of a watch.'

In August, 1828, at a time when there was a good deal of sickness in the garrison at Trinidad, this practice was adopted in the military hospital of that island; *that is to say*, they bled freely and used active purgatives in the commencement, and, afterwards, *the saline medicines were administered until the fever abated*, and during the convalescence, the quinine was given in large doses. In a communication which I received from Mr. Greatrex of the Royals, who at that time had charge of the hospital, he states, "that *the above system* has been applied to three hundred and forty cases, or thereabouts, including both the remitting and yellow fevers admitted into the hospital, after the fever had existed variously from six to seventy-two hours, antecedently to an application to the hospital, *with such success*, that during the last seven months not a case had died." This document is dated about seven months after the commencement of the above practice. Mr. Greatrex also states, that within that time three men died, having the remitting fever, but they had also abscesses in the lungs, and purulent expectoration. As these three cases were complicated, with extensive organic disease in the lungs, it is probable that they would have been fatal under any treatment. But out of the three hundred and forty cases of essential fever, which had been treated in the manner described, there was not even one death *in the Royals*, from the time that this practice had been adopted; and, I may add, that in the West Indies, Trinidad is generally considered as one of the most sickly islands.

"It can be clearly proved, that in the West-India fevers, the patients that are left entirely to themselves have a much better chance of recovery, than those who are treated with emetics, calomel or antimony, opium or acids; and that these remedies, instead of being useful, aggravate the sufferings of the patients, they decidedly increase the very evils that they are meant to relieve, and add greatly to the mortality from fever in hot climates.

"It is, I believe, an essential error, to consider fever as entirely a disease of the solids, and still more so to treat it solely with a reference to the mere state of excitement; at least, this much is certain, that those who attend only to the solids and mere increased action, can never cure even one case of the yellow fever that is really severe. Such cases can only be treated with success, when we reduce, by active treatment, the increased excitement in the commencement, and then prevent, by chemical means, those chemical changes in the blood which are, in reality, the source of the diseased action in the solids, and the true cause of the mortality in those fatal fevers. After having seen much to convince me that this was the fact, I adopted a mode of treatment widely different from that which I had formerly used; and in as far as it has yet gone, the use of the muriate of soda, the nitrate of potass, the carbonate of soda, the Rochelle salt, and other active saline medicines, given at a proper period of the disease, has been attended with a run of success, to which there is no parallel in the practice of those who attend merely to the nervous system, or in the hands of those who believe that fever is only an inflammation in the mucous membranes; and having so often seen the saline treatment fairly put to the test, I am induced to believe, that if this practice be generally adopted, the mortality from fever, in hot climates, will be greatly lessened: for so long as we considered fever as merely a disease of the solids, and treated it as such, we had no command over the disease; but from the moment that we considered fever as a disease of both the solids and the fluids, and met it accordingly, we have been able to prevent much suffering, to save many valuable lives, and in those islands where this treatment has been fairly tried, the hitherto fatal West-India fevers have, in a great degree, been disarmed of their terrors.'

As I have stated some circumstances in the above paper relative to the fever in Trinidad, it may be proper to make a few observations on this subject, particularly as those whom I had reason to believe would have supported me in the facts which I have mentioned, have acted in a manner very differently from what I expected.

In the month of July, 1828, we received intelligence in St. Thomas, that the yellow fever was raging in Trinidad, and uncommonly fatal, particularly amongst the military. As I had, at that period, given the saline treatment a fair trial in one island, I was anxious to ascertain if the same practice would be equally successful in another, that is generally considered to be even more sickly. With this view I left St. Thomas in the beginning of August, and went to Trinidad, with the hope that my doing so might be the means of saving many valuable lives.

The success of the saline treatment in the cure of fever, had made a considerable sensation in the Danish West India islands; and the captain of the man-of-war which conveyed me to Trinidad, had mentioned some circumstances relative to this at a dinner party, to some of the officers belonging to the Royals, in consequence of which, very soon after my arrival in Trinidad, I received a visit from General Farquharson, who was then Governor of that island. After having invited us to St. James's, he asked me, if I would have any objection to communicate my mode of treatment in fever, to the medical gentleman who had charge of their military hospital, observing at the same time, that the mortality amongst the soldiers was very great. In answer to this, I told him that I was no dealer in secrets; that my object was to make the practice which I had been using, as useful, and of course as public as I possibly could. On the same day I was informed by his Excellency, that he had seen Mr. Greatrex who had charge of the hospital, and that this gentleman would be most happy to see me there.

When I saw Mr. Greatrex, he told me that he had tried the various methods of treatment in general use; but that they had not been successful in that hospital.

After a very long conversation, he said, that he would give the saline treatment a fair trial, and let me know the result. We then visited all the cases, and the saline practice was immediately commenced.

As I had gone to Trinidad in a man-of-war, and under a promise not to remain more than a few days, of course nothing could be decided in so short a time. The saline treatment, however, was tried, and I shall soon prove that it has been most eminently successful.

I have reason to believe that, at first Mr. G. spoke very favourably of the non-purgative salts as remedies in fever, and continued to do so, until another individual, whom I have never seen, mixed himself up in this affair. They then began to talk of having combined the saline treatment with *other measures*, which rendered it doubtful how much was to be attributed to the one, and how much to the other. This, however, was not very satisfactory even to the officers, and then they made another discovery; namely, that after all the treatment which they were using was neither more nor less than the practice which had been recommended by Dr. Jackson, who was, we all know, an army practitioner. The treatment, however, which they used was as far from that recommended by Jackson, as the gulf of Paria is distant from the Ganges; and if Jackson himself could not cure fever with his own remedies, even in Spain, it would be rather strange if the same treatment should succeed with others in the West Indies.

I had heard the story about the *other measures* and the *Jacksonian practice*; but I could scarcely believe it, until the autumn of 1829, when I received the following document from Mr. Greatrex, in which he throws the saline treatment as much as possible into the shade, and dwells considerably on their other measures, as well as the mode of treatment recommended by Dr. Jackson. This, however, will have no weight with those who know how unfortunate they had been in Trinidad with their other measures, previously to the month of August, 1828, or how little the Trinidad army physicians appear to know about Jackson's treatment.

'*May* 12, 1829.
'*St. James's, Trinidad.*

'Dear Sir,

'Some time ago, Captain M........ 'showed me part of a letter he had received from you 'relative to fever, upon which subject it appeared that 'you had expressed a wish to hear from me. I ac'cordingly have much pleasure in taking the present 'opportunity, afforded by the captain's being about 'to forward a package to you, of attending to your 'desire.

'I have reflected on the statement you made to 'me of your theory of West India fever, made expe'riments with alkalies, *and administered the mixture 'composed of muriate of soda, nitrate of potass, 'and water in the proportions and at the times you 'pointed out*; but with what particular 'regret to say, I cannot determine for you, in consequence of its being mixed with the effects of several other measures which we are in the habit of adopting.

'I have been desirous to make out the precise and uncombined consequences on the living blood of your sodaic mixture; but I will give you facts. During the month following your visit to us, we admitted into the hospital from forty to fifty cases of the remittent and climate fevers, of which three died*; and I administered the sodaic mixture myself to a given number, in order to be sure that it had been taken. It was of that number that the three died. One of them was admitted within the first eighteen hours, another on the second day; the third on the third day. After death the blood was in a state of *solution*. Examination was performed in twelve hours after decease. The blood had not been *alkalised by the soda*; it was dark-coloured. The morbid evidences were precisely similar to those I showed you in the dissection reports. *These men had been in the habit of drinking immoderately of rum, and undergoing unusual fatigue, exposed directly to the solar heat.*

'By the action of the *alkalies* on the blood, ab-

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* [*Note by an Officer of the Royals.*] 'These men belonged to the Royal Artillery, and were conveyed a distance of two miles to the hospital, in the heat of the day, with the fever on them. A severe ordeal, I should say, for men who were much less sick than they were.'

'stracted from cases of remittent fever, it is plain enough, that there is much acid in it. On that of patients of the climate fever the same observation occurs; and on that of the healthy man, here at least so to all appearance, a similar effect results: but less in degree from the alkalies. I have not as yet tried the alkalies on the blood of animals in this locality so productive of malaria, which I presume is to be considered the cause of the altered condition of the blood. The remittent fever blood is *darker coloured* and thinner than the blood of fever cases in England. As you remarked, it seems also liquid, as if dissolved. It never presents a firm crassamentum, nor any of the buffed or cupped appearances, so commonly exhibited by fever blood in England. The same circumstances obtain in the blood of the climate fever, as you very appropriately designate the yellow fever *, also in the colour and consistence of the uncomplaining man's blood, generally speaking, but sometimes more florid, yet still being thinner than what healthy blood is in England. I have taken the latter as the healthy standard.

'A similar change from purple to scarlet, as by the admixture of the alkalies is occasioned in the blood, by the immersion of the patient in the hot bath, as seen by bleeding during or immediately after his removal from the bath. Do you think this might be explained by saying that the acid

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* I designated this to Mr. Greatrex as the *endemic* y fever of the West Indies.

'and aqueous parts of the blood have been driven off 'as perspiration by the hot bath? I purpose trying 'the effects of alkalies on the blood when I return to 'England, which may perhaps assist in coming to a 'conclusion.

'*The way we combat the fevers is agreeable to 'Dr. Jackson's system, so admirably set forth in his 'invaluable book:* that is, we place the patient in 'a hot bath, 110°; and when well heated and per'spires, open a vein, and take away from thirty-six 'to sixty ounces of blood. Lift the patient on the 'bed, and have him rubbed all over for ten or fif'teen minutes. The head is placed tolerably high. 'In most cases a sound sleep supervenes, which 'continues from one to five or six hours, during 'which the cutaneous exhalation continues. On 'waking fresh linen is put upon him and the bed. '*An enema of sulphate of soda* and castor oil, with 'water, is now administered, to the extent of two 'gallons, more or less, according to the degree of 'peristaltic motion of the intestines, which may be 'induced. The evacuation is a scybalous and 'blackish compound. *Your sodaic mixture is given 'every fourth hour from the time of his waking.* 'Now, or in a few hours, we administer the croton 'oil, agreeably to the directions published by Mr. 'Tegart in the "London Medical and Physical 'Journal," for August 1825; and the effects of this 'oil, as they have appeared in some hundreds of 'cases, which have come under my observation in

'Trinidad, fully support the high merits which Inspec-'tor Tegart has assigned to it. The night is passed 'sometimes with, sometimes without sleep. The air 'in the wards of the hospital is never suffered to 'become close or offensive. *Your mixture is con-'tinued through the night. The next morning, at 'six o'clock, in ninety-nine cases out of a hundred, 'even including what is termed climate fever, there 'is a remission of symptoms more or less decided, 'when we give from twelve to eighteen grains of 'sulphate of quinine in one dose, instead of three 'or four grains every fourth hour.* Never any un-'pleasant effects on the sensorium, as many authors 'have said, have followed the administration of the 'sulphate of quinine in our hospital. The alkalised 'toast-water *ad libitum, which Jackson advises,* is 'used as a common drink, as is also the *supertar-'trate of potass* dissolved in water. As the tempe-'rature of the day rises the symptoms seem inclined 'to recur, when we put the patient under a shower-'bath, give two or three shocks, place him in a cur-'rent of air, and place a cold wash to his head. The 'effect is decisive. On the third morning of his 'being in hospital, there is almost an intermission of 'symptoms, when we merely repeat the dose of sul-'phate of quinine. If any vomiting recur, we give 'another dose of croton oil, two drops. If any local 'pain, or head-ache remain, we apply from eight 'to eighteen cupping-glasses, and abstract from 'twenty to thirty ounces of blood; then, if 'sary, apply a blister. The cupping is generally 'sufficient. We were led to give a large dose of 'sulphate of quinine by a medical officer touching 'here, and telling us that it was given in that quan-'tity on the coast of Africa with the best effects, and 'such have been the effects here. Two doses are all 'that are necessary. On the fourth day of the pa-'tient's being in the hospital, convalescence is esta-'blished; when we give an ounce or two of porter 'frequently, in conjunction with light soup, an occa-'sional shower bath, and keep the alimentary canal 'free. Under the divine permission the above 'system has been applied to three hundred and forty 'cases, or thereabouts, including both the remittent 'and yellow fevers admitted to hospital, after the 'fever had existed variously from six to seventy-two 'hours, antecedently to an application at the hos-'pital, with such success, that during the last seven 'months not a case has died. Three men have died 'in that time having the remittent fever, but, at the 'same time, abscesses in the lungs and purulent ex-'pectoration.

'I shall be most happy to hear from you.

'I have the honour to be,
'Dear Sir,
'Your most obedient humble servant,
'E. Greatrex.

'Wm. Stevens, Esq., Surgeon,
'St. Thomas.'

Now, with respect to the other measures to which Mr. Greatrex refers in the above letter, the fact is, that these measures are only successful in the West-India fevers, when they are combined, at the same time, with the saline treatment, and this is probably as well known to Mr. Greatrex as it is to myself. I also, like every one else, had long been in the habit of using other measures; but so long as I trusted merely to these, I had been just as unsuccessful in my own practice, as the Trinidad physicians had been in theirs.

I have already given an outline of the treatment recommended by Jackson; and those who are acquainted with the writings of that author, know well that his practice was not only radically bad, but miserably unsuccessful. He used salts, to be sure, like many others; but if he occasionally used anything like proper practice, it was only by chance. As I have formerly said, he was a complete solidist; and in place of recommending the alkaline salts on a steady principle, he places his chief reliance on bleeding, alternate baths, vitriolic emetics; and after having deprived his patients of all hopes of recovery by these measures, he then poisons them with sugar of lead. Such was the treatment recommended by Jackson, and we know, from his own confession, how miserably unsuccessful this practice had been in his own hands.

There are two facts, however, which may be fairly inferred from Mr. Greatrex's letter. First, that they had not merely adopted the saline treatment, but after having tried it on a large scale, they were continuing to use it.

Secondly, That there had been an end to the mortality from the moment that they had commenced attending to the vitiated condition of the blood, and adopted the use of the energetic non-purgative salts, on purpose to remedy the vitiated condition of the vital fluid, which is so generally the chief cause of death in the West-India fevers.

In the paper which was read at the College of Physicians, I did not say that the Trinidad practitioners had trusted entirely to the use of the non-purgative salts; for, had they done so, they would have acted in direct opposition to the advice which I had given them. I merely, in that paper, gave an outline of their treatment as described in Mr. Greatrex's letter; and then, on the same authority, stated the result. *Such is the fact, and both these points they admit to be true.*

Had Mr. Greatrex been left to himself, my belief is, that he would have acted fairly, and sent home a candid statement of the facts to the Army Medical Board in this country; and had he done so, my conviction is, that many of the soldiers who have since died from fever in the other West-India islands might have been saved; but, in place of this, they allowed nearly two years to elapse, and then, at the end of this time, they come forward publicly with the old story of their other measures, and accuse me of want of candour, for no earthly reason, that I know of, except that I had not given an account to the College of Physicians of their awkward attempt to give the credit of the success to an army physician, who was even less entitled to it than either of the nurses who administered the remedies which were used.

There are two documents on this subject in the Medico-Chirurgical Review, for the 1st of January this year. One of these is the paper of Mr. Greatrex, which I have now published, as I no longer consider it a private document. The other is a letter from Dr. Hacket, which I would also insert, were it not that it is written in language which few would have written, and that any person of proper feelings would be ashamed to read. Those, however, who may wish to refer to this communication, will find it in the Medico-Chirurgical Review, which is, I believe, the only journal in Europe into which such a document could have gained admission.

On the 10th of December, 1831, and *previously* to the publication of these documents, two letters were written by the editor of the above journal[*]: the one was sent to the editor of the Lancet, and the other to the Medical Gazette. In this letter, he publicly announces the arrival of the said documents; and then, with a few flourishes, after his own fashion, about the melancholy disclosures which he had to make, 'injury done to the medical profession,' 'disgraceful conduct,' &c. &c., he adds, 'I shall publish the documents forthwith, leaving Dr. Stevens to justify his conduct, and support his hypothesis in the best way that he can.'

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[*] This gentleman has adopted the belief that cholera is not contagious, but produced by a diminution of bile, and that calomel is the best remedy for effecting the cure. The diminution of bile, however, is, like the diminution of urine, or the want of heat, merely a symptom; and as to his treatment, we know well that calomel cannot cure cholera. I observe, however, that even Dr. Johnson has lately been approving of quinine and carbonate of soda in this disease, yet I fear that he is still something in the same situation with Sangrado, when he was advised by Gil Blas, for the sake of curiosity, to try the effects of certain chemical medicines in fever, as, at all events, they could not be more unsuccessful with these, than they had been with their former remedies. Sangrado, however, objecting to this sensible proposition, says,—'Je ferais volontiers cet essai, répliqua-t-il, si cela
'tirait point a consequence; mais j'ai publié un livre où je vant
'frequente saignée et l'usage de la boisson: veux tu que j'

I had, then, no choice left, and soon after the said documents were published, I sent a copy of the following letter to the editors of each of the journals in which I had been so wantonly attacked, and that, too, by an individual whom I scarcely knew, and who, certainly, could not have had any personal cause of complaint against me.

DR. WILLIAM STEVENS *on the efficacy of Saline Agents in the treatment of West-India Fevers. Being a Defence of his practice against the Charges of* DR. HACKET, DR. JOHNSON, *and other Physicians.*

'SIR,

'I DO not mean to lessen the force of the serious

'decrier mon ouvrage. Périssent plutôt le peuple la noblesse et 'la clergé. Allons donc toujours notre train.' As there was, therefore, not only vanity, but the sale of a book in the question, it was mutually agreed that they should go on with their old practice. Such was the laudable conduct of Sangrado, and such has also been the case with Drs. Johnson, Searle, and most of the other practitioners who had formerly written on the Indian cholera.

charges which Dr. Johnson and my other more able opponents have preferred against me, by dwelling on subjects that are decidedly of minor importance in the present inquiry; but if you will have the goodness to compare Dr. Johnson's letter and the Trinidad documents with the paper which was sent nearly two years ago to the Royal College of Physicians, you will find, either that these gentlemen have but a sorry conception of my opinions with respect to the treatment of fever, or that Dr. Johnson, at least, has wilfully endeavoured to mislead the public on this subject, by giving a very unfair statement of my views.

'It has long been known, that some of the neutral salts possess the power of reddening the blood; but it was not, I believe, known, until lately, that *every one* of the alkaline salts possesses the property of striking a red colour with the black colouring matter, and of converting even the blackest venous blood into the brightest arterial; and, as a proof that this fact was not generally known, I may mention that one of the latest writers on the blood in this country, informs us that the muriate of soda darkens its colour. It is also a fact, that the Royal College of Physicians were so little prepared to admit the assertion, that *all the alkaline and earthy salts*, except those which contain a great excess of acid, redden the blood, as well as some other chemical facts which I had there stated, that they would not consent to the reading of that paper, until they had first sent it to one of th best chemists of the day[*], to ascertain, by actual experiment, whether the circumstances which I had stated as chemical facts were really correct.

'In the same paper I stated my belief, *that the natural saline ingredients of the circulating current were the true cause of the red or arterial colour of the blood; and whatever the colouring matter may be, that black is the natural hue of this agent; but that the whole of the alkaline salts possess the power of striking a rich arterial colour with this black substance.* I observed, also, that in the last stage of the yellow fever the blood, in bad cases, becomes black and vapid from the loss of its natural saline matter; but that, by adding a small quantity of any one of the neutral salts to this black fluid, we could again restore it to its red or arterial appearance, even when the blood that we used had been black as ink. The Trinidad physicians do not attempt to deny this; and Dr. Hacket admits that the blood is invariably *dark-coloured and diseased* in the West-India fevers, without there being even "*one single solitary exception to this rule.*"

'The fact, that the blood owes its red colour to its own natural saline ingredients, is now, I believe, almost universally admitted by those numerous physiologists who have witnessed the experiments which, in my mind, prove beyond all question that such is the case; but of this hereafter. I stated also, in the same paper, that acids, alkalies, electricity, and, in short, everything else that possessed the power of decomposing or interfering with the agency of the saline matter, destroyed the red colour of the blood, and made it perfectly black, as I believe, by decomposing or diminishing that which is the true cause of its red colour.

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[*] Dr. Prout.

'But it seems, notwithstanding all this, that the Trinidad physicians have discovered that *alkalies* redden the blood out of the body *; but complain that its colour had not been reddened by *soda* in three men who had never taken *even one particle of that alkali*, and who had died after having been carried to the Trinidad hospital half dead in the last stage of the yellow fever. This new discovery, however, that *soda* reddens the blood, either in or out of the body, is not correct; for the alkalies, as I have stated, do not redden, but blacken its colour.

'I have, for reasons which will afterwards be given, long adopted the belief, that carbonic acid, and not carbon, is the cause of the dark colour of the blood in the venous circulation. I have also long been aware of the fact, that the aërial poisons which cause fever, have *an acid effect* † on the blood; and both darken its colour and give it a deranged appearance, even before they excite the disease. It is probable that I did mention these opinions to Mr. Greatrex; but it was neither soda nor potass, nor any of the other alkalies, which I recommended these gentlemen to use: and, certainly, it is not my fault, if neither Dr. Johnson nor themselves be acquainted with the difference betwixt a pure alkali and a neutral salt. It may be quite enough, if I am made to be answerable for my own faults; but, surely, I am not accountable for the errors of others; and Dr. Johnson, who reviewed my paper on the blood, and ought to have known better, has no right to put nonsense into my mouth, which I never could have uttered; and after having stated that all the alkalies blacken the blood, they have no right to call upon me to prove that soda can redden it, even in opposition to my own facts.

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* See Hacket's Paper.

† See Mr. Greatrex's Paper in Johnson's Journal.

'They state, that soda had nothing whatever to do with the success in the above island. Now this is a mere equivocation; for *I never said that it had.* I am glad, however, to find that they admit the success, for by admitting this, they prove, at least, the one-half of my statement to be true. They state also that, after death, the blood of sodafied patients was found to be precisely similar to that of the unsodafied. Now, if Dr. Johnson had allowed himself time to have reflected on this subject, even for one moment, he would never have uttered such insufferable trash: for soda, as soda, or, in other words, in its alkaline form, had nothing whatever to do with the treatment of the Trinidad fevers. It was, as I have said, neither soda nor any of the other alkalies which I advised them to use; but the strongest, and, perhaps, the best, of all the non-purgative *neutral salts*,—namely, a saturated solution of the muriate of soda, or, as it is now called, the chloride of sodium, combined with the nitrate of potass. They admit that they did adopt the use of this remedy in consequence of my visit, and still continue, I believe, the use of it up to this day, *both in the proportions and at the times which I had pointed out;* and I will prove, that however numerous the deaths may have been previous to the commencement of this practice, yet from the moment that they began to attend to the diseased state of the blood, and adopted the saline treatment in that hospital, they had but few opportunities of ascertaining, *after death*, whether the blood had been *sodafied* or not.

' " Permit me," says Dr. Hacket, " to inform you, that every case that dies in our military hospitals must be opened and reported on; and, therefore, the opportunities of the military practitioner for examining the human body, and viewing the morbid changes in its structure, are perhaps greater than *that* of any private practitioner, however great or extensive his practice may be." Dr. Hacket, however, forgets to state that, however numerous the deaths may have been previous to August, 1828, yet from the moment that they commenced, not the *soda, but the saline treatment*, they had scarcely a death from fever: consequently, though I admit that previous to that time there had been dissections enough, yet I shall prove that from that period their opportunities of examining the dead bodies in the Trinidad hospital could not have been so very extensive as they had been before.

' I have already referred to the three cases belonging to the Royal Artillery, where the men had been brought to the hospital in the last stage of the disease, and where, after death, the blood had been, as they say, in a state of *solution*, but not sodafied. The latest letter which I have from Trinidad is dated the 4th of April, 1830; that is, about twenty months after the commencement of the saline practice. During the whole of these twenty months, the Royals had lost, up to that date, only eight men from fever, even including the three men mentioned by Mr. Greatrex, in his letter to me, as having died from organic disease in the lungs; and of the other five, two of them had died the same day that they had been brought to the hospital; consequently, in the first twenty months which followed my visit, they had only three opportunities of examining the bodies of patients who had been under anything like fair treatment. These are the only cases where they had an opportunity of examining the state of the blood after my visit; and these cases were probably not examined; or if they were, they have not given us any account of the result. These are facts; and from these it appears that, after the month of August, 1828, up to the 4th of April, 1830, that is, twenty months after they had commenced the saline treatment, their opportunities of examining the bodies after death, to ascertain whether the blood had been *sodafied* or not, have not been so very numerous as Dr. Hacket's statement would lead us to believe.

'I know one private practitioner, who, about the same time, but in a much shorter period, had three cases in his own establishment. One of these was his partner, the other his apothecary, and the third his clerk; they were all treated according to the old system, with bleeding, calomel, &c.; and out of these three cases of yellow fever, which he had seen in the very beginning of the disease, he had, in his own domestic establishment, as many opportunities of examining bodies after death, as they had in the Trinidad hospital, during the first twenty months after they commenced the saline treatment.

During the first month after my visit to Trinidad, from forty to fifty fever cases were admitted into that hospital; they were all treated with the *muriate of soda and nitrate of potass.* Out of this number three died; and all the rest, I believe, recovered. These three were the only cases in which they had an opportunity of examining the bodies after death; but I do not consider these cases as affording the slightest argument for or against the saline treatment; for these were not fair cases, either for this or for any other experiment. In the paper which was read at the Royal College of Physicians, I stated that "where proper means are used to protect the organs from the increased excitement during the early stage of the disease, and after the excitement is sufficiently reduced, when proper nourishment is given, and certain saline medicines are timely and judiciously used, the bad symptoms are generally prevented. When proper saline medicines are used th do not fret the stomach; they act on the intestines as much as is necessary; *they keep up all the secretions*, particularly that of the kidneys, whilst enough is absorbed to enter the circulation, and prevent the dissolution of the blood and preserve it until the fever abates, and all danger is past. This I am warranted to state as a fact, inasmuch as this treatment was commenced in the West Indies in 1827; and since then it has stood the test in several hundred cases of the West India fevers where it has been tried, both by myself and others; and with scarcely a single loss when we were called to the patients within the first twenty-four hours after the attack; and, with very few deaths where we were called in previously to the commencement of the fatal symptoms." But though I said this, I did not say that the saline treatment was capable of reanimating the dead, or saving soldiers from the grave who are brought to an hospital in the last stage of the yellow fever, with their fate already fixed from previous neglect. These men were, all three of them, notorious drunkards; they had all of them been undergoing fatigue while exposed to the intense rays of a West India sun, and that too during the hottest season of the whole year. They had been taken ill at a distance; they had not been bled in the beginning; they had been carried to the hospital during the heat of the day; and were exposed, on their way to St. James's, while the fever was on them, to the direct rays of a burning sun. Two of them were brought into the hospital at a late period of the disease; and from these circumstances, but above all from their not having been bled in the first stage, it is more than probable that some serious injury had been done to the brain; and that the finger of death was already upon them, before any attempt to arrest its progress could have been made. It is also admitted, even by the Trinidad physicians themselves, that it is only within the first twenty-four or forty-eight hours that good can be done by any treatment in these fevers; for, as they say, after this the disease becomes, as it were, a labyrinth most difficult to unravel; consequently, these three cases do not, in my mind, afford even the slightest proof against the saline treatment, or anything else, except neglect in the beginning, and the imprudent manner in which these men had been brought to the hospital.

Twelve hours after death, the blood in these cases was found to be in a state of solution, but dark in colour. Now the truth is that, in yellow fever, death may be produced in one or other of two different ways. In the first place by injury done to the brain, or to some of the other important organs, during the violent excitement which generally occurs in the first or inflammatory stage of the disease. But even when they escape this, death may be caused solely and entirely by the diseased state of the vital fluid. When they die from the first cause, the blood may still be in a state of solution—as is said to have been the case with these men—but still it is diseased. I am not quite sure, however, that in these cases the diseased or unsodafied state of the blood was the cause of death. It is true, that in these men, saline medicines had been thrown into the stomach; and that too at a time when this organ had probably ceased to perform its functions. But, has it never occurred to these gentlemen, that if these men had escaped the injury, which had probably been inflicted in the brain in the first stage of the fever, and if the diseased properties of the blood had been properly remedied by the saline agents, they would not then have had an opportunity of examining their bodies after death, to ascertain whether the blood had been acted on or not by the neutral salts? for these men, like the others, would not have died if they had been properly bled in the beginning of the disease; and, after this, had the saline medicines been given at a proper period, when they could have entered the circulation, in time to prevent those fatal changes in the blood, which are in reality, in these fevers, but too often, though not always, the sole cause of death.

'The Trinidad army physicians appear to be perfectly aware that the saline treatment was the cause of the great success in that island; and, in point of fact, they admit almost as much; but they take great pains to conceal the circumstance, that they had learned this practice from me; and the manner in which they do this is the only ingenious part of their whole proceeding. They use the word *saline* as seldom as possible; and endeavour to deceive their readers by talking eternally about *soda, or other alkalies;* and try to convince the public that these were the remedies that I had recommended them to use.

'Dr. Hacket confesses that he is stupid enough not to comprehend how *soda* should produce a similar effect on the blood in the living body, to that which occurs when soda is mixed with the same fluid out of its own vessels. He observes also, that "considering, too, the very minute quantum of soda that can possibly pass from the stomach, direct and unchanged, into the mass of circulation, how small must be the means to produce such mighty effects!" Now, what will the reader think of all this, who has seen by their own statement that these gentlemen had never used even one particle of soda; and, however successful this sort of logic might have been with Mr. Greatrex, who was under his command, yet is it not an insult on the common sense of their medical readers to talk in this way, in the very same paper in which they admit that it was neither soda nor any other alkali that I had recommended them to use, but the strongest of the non-purgative neutral salts? Is there even the merest tyro in the profession who does not know that alkalies and *neutral salts* are, in their effects, as opposite to each other as day is to night, but, above all, in their effects on the blood; for the former, as I have said, blackens arterial blood, and the latter converts even the darkest venous into the brightest arterial? Now, if all this be true, it then follows, either that these gentlemen are grossly ignorant of this, or that they have tried to deceive the profession by the same sort of reasoning which they had found, perhaps, to be partly successful with those who are totally unacquainted with medical subjects. I will not for the moment say one word about soda, for, with this, in my practice, I had nothing to do, nor they either in theirs; but, with respect to the action of the neutral salts on the blood, my belief is, that this sort of scepticism may be carried a great deal too far. That the salts do enter the circulation is beyond all question; for healthy blood, in every part of the world where it has been analyzed, has been found invariably to contain a given, and that too a large, proportion of saline matter, to which it is indebted for its red colour, and in some degree for its stimulating power; and without this, the blood, as in cholera*, or the last stage of the yellow fever, becomes totally incapable of performing its functions.

'If Dr. H. really knows anything in his profession, he ought to be aware that when strong salts, such as those which they used, are taken into the stomach in excess, that the greater part enters directly unchanged into the circulation, through the medium of the vena portæ; and produces an instant effect, and suddenly changes the properties of the blood; and when used in the last stage of fever, a part of the salts is retained in the circulation, particularly of those that are natural to the blood, while the excess, after having passed through the circulating current, is removed by the secreting organs without having been decomposed either in the stomach, or changed in its properties by the vital principle. Had Dr. Hacket taken away small quantities of blood in the last stages of the yellow fever, from patients who had been treated under the old practice, as recommended either by Jackson or Johnson, he would have found that the blood every hour was becoming more diseased in its properties, and blacker in its colour, chiefly from the loss, or at least from the great diminution of its natural saline ingredients; or, had he made the same experiments on patients who had been treated with the active salts, such as they used in consequence of my advice, he would have found in that case that the blood was hourly becoming not merely more red in its colour, but less diseased in its appearance; or, had he injected a small quantity of the same mixture into the vein of an inferior animal, he would have found that even in living vessels the vital principle presents no obstacle to the reddening of the whole mass of blood. He would have found also that the action of the heart was not only augmented, but that all the secretions were immediately increased; and this even he himself admits to be a great object in the cure of fever. If, on the other hand, he had adopted a false theory (which has already been the cause of great mortality) used any of the acids, as Chisholm and others had done, with the view of reddening the blood by means of their oxygen, he would then have found that the nitrous and other acids which contain oxygen in a state of loose combination, not only blacken the blood out of the body, but when used in the last stage of the yellow fever, kill, not so quickly, yet cause death as certainly as if the patient had received a bullet through the head or the heart.

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* See Dr. O'Shaughnessy's analysis of the blood in cholera, in the Lancet and the Medical Gazette.

'The Trinidad army physicians talk of having used the sulphate of quinine in the yellow fever, but forget to tell us at the same time that this was neither a discovery of theirs, nor a new remedy in 1828. The practice of giving this salt in the yellow fever was commenced by the French physicians in the island of Martinique, and with considerable success, as far back as the year 1823. The sulphate of quinine, like all the other alkaline salts, reddens the blood out of the body, but it possesses this power in a much inferior degree to the muriate or the carbonate of soda, the nitrate, the chlorate, or the carbonate of potass. The sulphate of quinine has been long used, both by myself and others, in the islands of St. Thomas, St. Croix, &c., and we have found it a useful remedy, but we could only trust to it during the convalescence; and even the Trinidad physicians very properly did not use this remedy until, in ninety-nine cases out of a hundred, the patients were out of danger; that is, until the diseased state of the blood had been previously corrected by other salts, that are much stronger in their saline property.

'Dr. Hacket states, that when I visited the hospital at Trinidad, I admitted the similarity of their treatment to my own. Now this is a mistake; for I knew, from the rate of the previous mortality, that there must have been some radical error in their practice. I saw clearly what this was, and pointed it out to Mr. Greatrex. They make me to say, also, that the only difference in our treatment could not be great, for that we were both in the habit of using *alkalies*. Now this is also a mistake; and I again repeat that it *was not alkalies*, but the non-purgative neutral salts, which they afterwards used in consequence of my recommendation. They say that even before my visit they had used the supertartrate of potass as recommended by Jackson: this, however, is a most improper remedy, particularly where there is, as in these fevers, an excess of acid in the stomach. It is also true, that this and other salts, which are formed with vegetable acids, are decomposed in the gastric organs, and, consequently, are not to be trusted to in these diseases. The Trinidad army physicians, however, appear to be totally ignorant of this. Dr. Hacket states, that it was the wish of Mr. Greatrex to employ the stronger salts as recommended by me, and that he had consented to their use. He is not perhaps, however, aware how many lives were probably saved by this condescension on his part. It is also stated, that I approved of bleeding in the warm bath, and that this in the practice of both w our *sheet-anchor*. Now, though I will not follow the improper example of these individuals, and assert that this is a misrepresentation, yet I do say, that in saying this they have fallen into a great error; for the fact is, that though I consider the free use of the lancet as essential in the first stage of the yellow fever, and said so to Mr. Greatrex, yet, with respect to the warm bath my conviction was then, and still is, that immersing patients in hot water is a most improper measure, particularly at the period at which they frequently used it; that is, in the first or inflammatory stage of that disease.

'In an epidemic of the yellow fever, which commenced in the island of St. Croix in July, 1817, after having found the mercurial and other methods of treatment in common use to be of no value, I then gave the warm bath a fair trial; but in every case that I used it the patient died; and from that moment to this, to the best of my recollection, I have not, even once, used this measure. As I was anxious to prevail upon the medical gentlemen at Trinidad to give my treatment a fair trial, I had no wish to offend them by finding much fault with theirs. But, under the above circumstances, I could scarcely have said that bleeding in the warm bath had been my *sheet-anchor* in St. Thomas's; for it is well known in that island, that, in place of *this measure*, I invariably used *iced water* when it could be procured, for the purpose of removing the morbid heat from the body. I told Mr. Greatrex that ever since I had adopted the *saline treatment* I had not lost even one case, where I had seen the patient early in the disease. In place of this, they make me to say that I had lost no case of fever since I had adopted the practice of *bleeding in the warm bath.*

'Dr. Hacket does not deny that Trinidad is a sickly island; and admits, that the cases which are met with there are of "the severer type of tropical fevers." In a document, which is dated Tobago, 23rd of October, 1830, he states that the fevers which he had been accustomed to see *in Trinidad, for the last five years, were of the aggravated congestive form;* and they do not pretend to say, that the cases that occurred during the first twenty months after my visit were more *mild* than those which had been met with previous to that period: but they forget to state, that *before this,* under their old Jacksonian practice, the mortality had been great, and that, *after this,* they were most successful. Now there must have been some cause for this sudden change; and though I may admit for a moment that my theory may be wrong, yet when we find out a method of treating the West India fevers, where, in the garrison of a large and sickly island, during the period of twenty months, out of probably about from seven to eight hundred cases* of the various fevers to which the soldiers were subject, and out of this number there were only about eleven deaths, and of these eleven only three died where the men had been under anything like fair treatment,—if this be true, and I believe that it is, we need not quarrel with the saline treatment, merely because there may be, as yet, some little doubt about the modus operandi of these agents.

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* It will be seen afterwards that this is considerably less than the actual number.

'But it seems now, that the saline treatment had been combined with other measures, which rendered it doubtful how much of the success was to be ascribed to the one, and how much to the other. But may I be permitted to ask, at what particular period did these gentlemen become acquainted with these other measures? Did they know of them before the month of August, 1828; and, if they did know of means previous to that period, by which they could cure or lessen the mortality of the West India fevers, was it not criminal in them to allow the mortality to continue up to the very moment of my visit to that island? And if they did know, previous to that period, of means or measures for preventing the mortality, what excuse can they make for having allowed so many of the soldiers to die? For there is one most important fact which these gentlemen have entirely forgotten to mention, namely, *that the mortality had been very great in that garrison up to the very moment of my visit.*

'From the beginning of that year, up to the middle of August, it is a fact, that the Royals alone lost, in that hospital, upwards of forty men from fever *.

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* It was in the first battalion of the Royals that this number

'It is now, I believe, generally admitted, that the blood is a living fluid; and if so, it must then, like everything else that possesses life, be subject to disease. It is also true, that the mortality ceased only from the moment that I had not only directed their attention particularly to the circumstance that, in the fevers which they had to contend with, *the diseased state of the blood was the chief cause of death*, but pointed out the means by which they might greatly lessen the mortality, by preventing those fatal changes in the vital current, which are, in reality, in these fevers, the great cause of the sudden dissolution. It is also true, that from that period they scarcely lost even a case from fever, except the three men already mentioned, and these men had been beyond all redemption before they were admitted into the hospital, as well as the other two who died on the very day that they were brought in. But I know well what their practice was, both before and after the middle of August, 1828; and I know the result. I know also what those other *unnecessary measures* were which I did not recommend, and which they combined improperly with the saline treatment; apparently for the purpose of taking to themselves as much of the credit of the success as they possibly could. They knew, however, of all

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was lost: their actual strength was, I believe, about three hundeed men; consequently, in the Royals alone, they had lost about one-seventh of their number within a few months previous to my visit. So much for croton oil, active measures, and their old Jacksonian treatment.

those other measures to which they allude, previous to my visit, yet they allowed the mortality to continue up to that very period; but I will afterwards prove, that some of these measures were too insignificant to have had any effect on the result, while others, but particularly the hot bath, were not merely inert, but actually improper, and, above all, when employed at the period at which they used it.

'But it seems that the hot bath also reddens the blood. Be it so! and, whatever Dr. Johnson may think of it, even this is no discovery of theirs: but, for this very reason of its reddening the blood, the hot-bath ought never to be used during the hot stage of a burning fever. It is our duty at this period of the disease not to redden the blood by a hot bath, but to reduce the excitement by means of the lancet, and if the one be proper the other must be quite the reverse. Fortunately, however, they used this measure *only once*, and that too, as I have said, during the hot stage of the climate fever*, when the blood was already but too red; and then, in the dangerous stage of the disease, when there was great risk of death from sudden diseased changes in the vital current, they did not then trust the reddening of the blood to the hot bath, or to soda either, but to the strongest, and, perhaps, the best, of all the non-purgative neutral salts. This is what they did trust to after my visit, for preventing the blood from becoming black or more diseased in its properties: and, after this, they had, as I have said, scarcely even one opportunity of examining the bodies after death. Had they not, however, used the saline treatment, it is very probable that their dissections would have continued, after the middle of August, to be just as numerous as they had been before.

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* This is also a disease of the blood, but it is not produced by a *poison;* and one of the characteristic marks of the climate or seasoning fever is, that the blood in the veins is almost as florid as that in the arteries.

'That the mortality was lessened after the middle of August, 1828, is beyond all question; not that the season became more favourable for the British soldier; for August, September, and October, are frequently the three worst months in the whole year; and in twenty months these soldiers had been exposed to every variety of tropical seasons. After this period the mortality ceased, partly, as I believe, because I had warned them against the improper remedies recommended by Jackson, Johnson, and a multitude of others; but, as I have said, chiefly to the circumstance of my having not only *drawn their attention to the diseased state of the blood as the cause of death*, but pointed out the proper means by which this fatal effect could be prevented, in almost every instance where the patients were brought to the hospital early in the disease. These gentlemen admit that great success did follow my visit to their hospital; but, like Corporal Trim, they appear to be "piqued for the reputation of the army," and claim the sole merit of this to themselves, except what they are pleased to give to Dr. Jackson, Inspector Taggert, and other army physicians. Their great dependence, they say, (or one of them at least,) both before and after my visit, was "promptness and decision, bleeding in the warm bath, castor oil, and quinine." Now these, *except the warm bath*, are all very proper; but these of themselves are no more a match for the ardent and malignant fevers of the West Indies, than the mercurial practice of Dr. Chisholm, or the leeches and gum-water of M. Broussais. And may I ask, if Drs. Jackson, Johnson, Mr. Taggert, or any one else, had taught them how to cure the West-India fevers previous to my visit to that island; or, if the warm bath, castor oil, vitriolic emetics, calomel, sugar of lead, quinine, cream of tartar, or alkalized toast-water, can cure these diseases, may I again ask why they allowed so many of the soldiers to die, and why their dissections were so frequent, up to the very moment of my visit*?

'Dr. Hacket, after favouring us with an account of their other measures, gives, I must say, a most unfair view of the nature of the practice which has been attended with such signal success; he states that the sole cause of the cure may as well be attributed to any one of the other means, as that a *little soda* had caused all the advantages which they had obtained, that is, after my visit. The reader will himself see the art with which all this has been got up. Had they trusted in reality to a little soda, they would not have been more successful after my visit than they had been before; or if Dr. Hacket, who is, I believe, still in the West Indies, will only return to Trinidad, and again give their other measures and a little soda a fair trial, he will find that, without the assistance of the active saline agents which they have since used, their old Jacksonian practice and other measures will again be just as unsuccessful as they had been previous to the commencement of the saline treatment.

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* The statement which I have given of the previous mortality under the old Jacksonian treatment, is made on the authority of an extract from the regimental books, about the correctness of which there can be no question.

'When Dr. Johnson first announced the Trinidad documents, he promised to lay before the profession information of the greatest importance. That they do contain facts of great value is perfectly true; but these do not prove exactly the points which he wished them to establish; for, so far from proving that the saline treatment in fever is of no use, they prove, beyond all question, that this is by far the most effective practice that has yet been tried, and infinitely superior to calomel and the other improper remedies which he has been recommending to the public. There is also another important fact contained in these documents—namely, that the same treatment had been tried, and with a similar result, in a fever of a fatal character which appeared in the island of Tobago in 1830. As to the other parts of their information, if there be anything good in these documents, it is merely a repetition of what I have stated in my paper on the blood. These gentlemen have evidently benefited, both by my theory and practice; but I confess that I can see nothing else, for even Dr. Johnson to admire, except the temerity with which one of them, at least, misstates the facts, and the great ingenuity which both of them have shown in endeavouring to conceal from the public the true cause of the great success which certainly followed after they had adopted a new practice, and treated fever on a new principle. As to the rest, their papers are actually beneath all criticism. They have given an account of the derangement in the solids, such as they had met with after death; but they do not seem to be aware of a fact, which I will afterwards endeavour to prove, namely, that all this derangement in the solids is merely an effect of the diseased state of the blood.

' I have stated, " that in a malignant form of the yellow fever, which is sometimes met with in the West Indies, the blood was nearly as thin as water, and, literally speaking, black as ink." In fact, it was so much dissolved that, even before death, it could scarcely be retained in its own vessels; and, when taken from the heart after death, it was so black that it could only be distinguished from the black vomit by a chemical test. All this is known to be true, to others as well as myself; and even Dr. Hacket admits, that during the disease the blood is sometimes so thin and dissolved, that it oozes from the gums, eyes, &c., even before death, as stated by me. They admit, also, that this state of the blood, which I have described as peculiar to a malignant form of the yellow fever, "which is *sometimes* met with in the West Indies," had also been observed in the military hospital at Trinidad; yet, after having admitted this, *which I have said,* to be true, they make me to say, on their own authority, that I have asserted that this appearance is *never absent;* and, on the strength of this, their own misstatement, they venture to impeach me for want of candour. But the truth is, that if these gentlemen had only been good enough to have confined their impeachments merely to what I *have* stated, they would then have had *nothing* to impeach; and finding this to be true, they fix *assertions* upon me which I have not made, and then, on these their own misstatements, and these only, they have the temerity to accuse me of errors which I have never committed.

'Whatever the immediate cause of the essential fevers may be, it is very clear that this cause must invariably exist. Inflammatory affections of the brain, or derangements of the stomach, which are often, *but not always,* met with, are merely accidental; and when they do exist, they are only an effect of the diseased condition of the vital current. In the paper which was read at the Royal College of Physicians, I stated my belief, that derangement of the blood was the immediate cause of fever, and that death, in *bad fevers*, was *often*, but not always, caused by its dissolution. Dr. Hacket admits that, on examination after death, "the appearances the blood presented were invariably, and without a *single solitary exception,* dark-coloured, viscid, and grumous." That this is often the case, in certain forms of the yellow fever, I have long known to be true; for, under the old treatment, patients frequently died before the whole of the fibrin was completely exhausted, and the imperfect coagulation of this gives to the blood a grumous, or, perhaps, even when exposed to the air, a viscid appearance. All this, therefore, is nothing new; and I will afterwards convince even Dr. Hacket, that the diseased state of the blood in these fevers was better described than he has done it, long even before he was born; but admitting every word he has stated to be true, yet it only confirms, and does not contradict even one syllable of, what I have said, namely, that a deranged state of the blood is the cause of the disease, and a dissolved or diseased state of the blood was, in these fevers, often, but not always, the cause of death.

'I have said that, in these fevers, the blood is invariably diseased. Dr. Hacket admits that there is not even a *single solitary* exception to this rule; yet he denies my statement, which is similar to his own, and then he appeals to others to approve of his veracity; and refers even to Dr. Johnson himself, who, I believe, has never seen the West-India fevers, and who is, I may venture to say, totally unacquainted with this subject.

'In the paper which was read at the Royal College of Physicians, I stated that acids, alkalies, electricity, and everything else that decomposes or interferes with the agency of its saline matter, makes the blood black and vapid. I have also formerly stated, that those aërial poisons which act as the remote cause of fever, have an acid effect on the blood, and not only blacken its colour, but derange its properties, even before they excite diseased action in the heart or any of the solids. I believe, also, that the black colour in the circulating current is not produced by an excess, or by a diminution, of the natural carbonic acid of the venous circulation, for this blackness exists even in the arterial blood. This darkness is, as I believe, caused in the first period of the disease, by the effect of the poison on the blood acting as an acid, or perhaps even as an alkali, and darkens the colour by destroying or decomposing the saline matter. The blood, however, becomes more red during the hot stage; but these fevers run their course with great rapidity, and in a very short period the blood becomes completely black from the loss, or at least from the great diminution, of its natural saline matter, which is, as I have said, the true cause, not only of its red, but arterial colour. Now, when the blood has almost entirely lost this, as in the last stage, it is not by acids, or vitriolic acid emetics, sugar of lead, or even by agents that contain oxygen in a state of loose combination, that we can restore the vital fluid to its healthy appearance; for this object can only be effected by throwing those saline ingredients into the circulation which are essential to its healthy properties, and are not only the true cause of its red colour, but one chief cause of its stimulating power. These have also, as I will afterwards prove, not only a specific effect in preventing or destroying the action of the aërial poisons, but they are, in fact, more essential to its healthy state, than either the fibrin, the albumen, the colouring matter, or any of the other ingredients of the circulating current, except, perhaps, its vital principle. These are some, at least, of the opinions which I have adopted, and will only relinquish them when I am convinced that they are wrong. Yet Dr. Hacket, who appears not even to have read my paper on the blood, undertakes to criticise it; and gives, I must say, a most garbled and unfair view, both of my theory and practice; after this he easily, and, as he seems to think, very cleverly, knocks down the fabric of snow which he himself had made, and to show the very great extent of his reading, he gives a learned quotation from a very rare work, namely, Dr. Paris's Pharmacologia, in which it is mentioned, that during the prevalence of a fever at Leyden, certain physicians in that place had consigned a great number of their patients to an untimely grave, from a belief that a prevailing acid was the cause of the disease; and, consequently, they had lost their patients from having trusted the cure entirely to absorbents and testaceous medicines. Now in answer to this, which Dr. Hacket pretends to be quite sufficient to upset the whole of my views, I have only to say, that though this sort of logic might have satisfied Mr. Greatrex, who had, like himself, an interest in believing, or, at all events, in trying to make others believe, that they were solely and entirely indebted to themselves for their own success; yet this kind of reasoning will scarcely be satisfactory to any well-informed members of the medical profession, particularly to those who have seen that the Trinidad physicians, by their own admission, allow that the remedies which I had recommended them to use, and which they did use, were neither *absorbents* nor testaceous medicines; and had they attempted to redden the black and saltless blood, which we find in the last stage of the yellow fever, with chalk, or any inert testaceous matter, in place of the active non-purgative saline agents which I had recommended them to use, they would then have been just as unsuccessful as the physicians of Leyden in 1699; or as they themselves had been in Trinidad with their other active measures, combined with their old Jacksonian treatment, previous to the month of August, 1828.

'Whether the saline treatment was or was not the cause of their great success, or whether the treatment which I have recommended be or be not a better practice than that of either Jackson or Johnson, may still be with some a matter of doubt. The Trinidad physicians may think that it is not better: I think it is; but this is a mere matter of opinion, on which I will still retain the right of thinking for myself; and they have as little, perhaps less, ground for accusing me of want of candour for thinking that it is, than I might have for charging them with misrepresentation for pretending to think that it is not.

'It appears that Dr. Hacket is only acquainted with the contents of my paper on the blood, through the medium of Dr. Johnson's Journal; or perhaps it was through the medium of the Doctor himself that Dr. Hacket received information respecting those misrepresentations and *pointed assertions*, on the strength of which he has founded his attack, and which, I do say, are not to be found in that paper. It is only, however, after he had already made a most unfounded, and I must say a most ungrateful attack, that he then states, "it is painful to me thus to express myself; if I do an injustice by my interpretation of Dr. Stevens's statement, I am truly sorry for it, and would call on him to correct it, or to set me right."

'Now as this is certainly being very generous, I can only say that I am in possession of more facts on this subject than these gentlemen are probably aware of, and before I have done, the chance is, that *I will set them right*; and when Dr. Hacket finds that in place of the *misrepresentations and pointed assertions* which he says I so unblushingly made in the paper which was read at the Royal College of Physicians, and on the strength of which they have founded their attack; what will he say, when it is proved, and it will be proved, that they themselves have admitted every syllable to be true which I have stated in that paper? But, above all, what excuse can he make, when it will be proved, I trust to the satisfaction of the public, that even if I had made a claim to a share of the successful treatment of fever, in that island, I might have done so with the most perfect propriety?

If this be proved, and I trust that it will, even to their own conviction, is it not then probable that one of them at least will repeat his own poetical quotation, and wish that he had coined his heart, and dropped his blood for drachmas, rather than have been guilty of such ungrateful conduct towards one who was willing to allow them even more credit than they had any claim to?

'It has been asserted by Dr. Johnson, that I have founded the superiority of the saline treatment on the result of this practice in Trinidad. This, however, is not quite correct, for the previous result of my own practice in the island of St. Thomas was of itself more than sufficient to establish the superiority of the saline over the old mercurial, or, in fact, any other treatment that has yet been tried: even from the first I considered the Trinidad practice as a mere secondary concern. At the time that my paper was read at the Royal College of Physicians, I was then on the eve of a long and unexpected visit to the United States of America, and as I could not at that period publish the whole, I gave merely an outline of the facts; but I did not pretend, in that short paper, to give a minute detail either of my own or of their practice, I merely gave an outline of the facts, chiefly with a view of calling the attention of the profession to an essential part of the living body, which I do think has been too much neglected; and, unless we attend to this more than we have done, my conviction is, that in so far as relates to fever, we shall probably continue to go on in the dark to all eternity.

'I may now state, that it was only in the year 1827, and after a long residence within the tropics, that I found out what I believe to be the fatal error which had been, in reality, up to that period, the true cause of at least one-half of the great mortality from fever in the West India islands. It was then that I commenced a new practice on a new principle, and the success was such as to be almost incredible; for from that period there was scarcely one death from fever in the island of St. Thomas, except in the practice of those who were unwilling to admit any improvement which they themselves with equal opportunities had not made. After this period, those who adhered to the old practice continued to be most unsuccessful. One of these gentlemen lost, at one period, in some cases three out of four; whilst, in the same epidemic, Dr. Stedman and myself scarcely met with a loss, after we had adopted the saline treatment.

'In 1828, after I had given this practice a fair trial in the island of St. Thomas, we received intelligence that the yellow fever was raging in Trinidad, and particularly fatal amongst the soldiers. It was then that I put myself to some inconvenience, and went to that distant island in the very middle of the hurricane months, with the hope of being able to prevail on the army physicians of Trinidad to adopt the method of treatment which I had found to have been so successful in another island.

'On my arrival in Trinidad, and after having explained the nature of my practice to Mr. Greatrex—I may be mistaken, for I quote from memory and from a conversation which occurred more than three years ago—but, in so far as I recollect, he told me that they had tried the various modes of treatment, as recommended in books, but that none of them had been successful; and that under these circumstances he should feel himself perfectly warranted in giving a fair trial to any practice that had been found to be successful in another island. Mr. Greatrex, who at that period had charge of the hospital, promised that he would give the method I recommended a fair trial, and let me know the result. It was not, however, until after a considerable period had elapsed, that I received any information from that island, and even then it came in an indirect way. It was only about the end of that year that one of the merchants of St. Thomas showed me a letter which he had received from a friend of his, a Captain in the Royals, dated Trinidad, 15th December, 1828, of which the following is an extract:—"If you know Dr. Stevens, pray give him my very best regards, and tell him that since he was here (four months ago yesterday) the Royal regiment *has not lost a single man*, which I attribute mainly to the lesson which he gave to our assistant-surgeon, and for which God bless him."

'I had had the pleasure of meeting the writer of the above letter, as well as most of the other officers in that garrison during my short residence in Trinidad, but I had never seen him before, and I have not seen him since. It is true that he is not a *medical authority*, but his opinion is, perhaps, not the less valuable on this account, for he was not an advocate for any particular theory or practice of his own. He was not the partizan of either party; he was guided only by facts and his own common sense. He knew that under their old treatment, and previous to my visit, they had been most unsuccessful; for the battalion to which he belonged had lost, from fever, about one-seventh of its actual strength within a few months previous to that period; he knew also that they had adopted a new method of treatment, in consequence of my recommendation; he knew that from that period the mortality had ceased, and from these premises he had drawn his conclusions—premises which I believe to be more correct, and more in accordance with the facts, than the conclusions which have been drawn by the medical gentlemen.

'I believe I have formerly stated, that in a letter which I received from Mr. Greatrex, dated Trinidad, May 12th, 1829, he admits that they had adopted the saline treatment, in consequence of my visit to that island. *He says that they had used a solution of the muriate of soda and nitrate of potass in the proportion, and at the times, that I had recommended.* He states that this my *sodaic mixture,* as he calls it, was given even *during the night,* and continued until in ninety-nine cases out of a hundred (including cases of the yellow fever) the fever had abated. And now when the danger was past, they gave two large doses of the sulphate of quinine during the convalescence.

'The great mortality previous to the month of August, 1828, and the result after, prove that there must have been some essential change in their practice. It was not merely that the soldiers had all of a sudden become more seasoned to the climate; for the same treatment had been previously tried and equally successful in the island of St. Thomas, even in robust sailors and other strangers who had just arrived from a cold country. In the same letter in which Mr. Greatrex admits that they had adopted the saline treatment, he states,—" the *above system*" (that is, the new system in which the use of the active non-purgative neutral salts constituted the great and the only essential difference betwixt their new and their old practice) " has been applied to three hundred and forty cases or thereabouts." Now this was the number of cases which had occurred since the period of my visit, and on which the new system had been tried, " and out of this number during the last seven months, there had only been three deaths from fever, and consequently three hundred and thirty-seven recoveries."

'I had received Mr. Greatrex's letter previous to the period that my paper was sent to the Royal College of Physicians, and in that paper I merely stated, that " in August, 1828, at a time when there was a good deal of sickness in the garrison at Trinidad, *this* practice was adopted in the military hospital of that island;" *that is to say,* they bled freely, and used active purgatives in the commencement, to reduce the excitement; afterwards the saline medicines were administered until the fever abated; and during the convalescence, the quinine was given in large doses. In the communication which I received from Mr. Greatrex, of the Royals, who at that time had charge of the hospital, he states "that the *above system* has been applied to three hundred and forty cases, or thereabouts, including both the remitting and yellow fevers admitted into the hospital, after the fever had existed variously from six to seventy-two hours antecedently to an application to the hospital, with such success, that during the last seven months not a case had died." This document is dated about seven months after the commencement of this practice. Mr. Greatrex also states, that within that time *three** men died having the remitting fever; but they had also abscesses in the lungs, and purulent expectoration. As these three cases were complicated, with extensive organic disease in the lungs, it it probable that they would have been fatal under any treatment. But, out of the three hundred and forty cases of essential fever, which had been treated in the manner described, there was not even one death in the Royals from the time that this practice had been adopted; and I may add, that in the West Indies,

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* Dr. Johnson, in his version of Mr. Greatrex's letter, leaves out the word three; thus leaving it to the imagination of the reader to suppose any number that he pleases. This may be an error of the press; but I think proper to notice it, lest it might be supposed that the error was on my side.

Trinidad is generally considered as one of the most sickly islands."

'Now will the reader believe, that in the paper to which the army physicians refer, this is every syllable of what I have stated with respect to Trinidad; or, after all the improper language that they have used, will it be credited, that they themselves admit every syllable of this to be true? Dr. Hacket admits that this is "*really their practice;*" and Mr. Greatrex admits the result to be correct as stated by me. I did not say, as I might have said, that I had gone to that island, with the hope of being able to lessen the mortality, by prevailing on the army physicians to adopt a practice which I had found to be so successful in St. Thomas. I did not say that the saline treatment, or any other, was the cause of the great success which followed my visit to their hospital. I merely stated, that such was their practice, and such the result. But if I did not say, then, that the success which followed my visit to that island was in some measure owing to that circumstance, *I do say so now, with the most perfect conviction that such is the fact.*

'If I did not, in that paper, notice the error into which Mr. Greatrex had fallen, about the blood not having been reddened by soda in the men, who had never taken even one particle of that alkali; or about his calling the saline practice, as he had used it, the treatment of Jackson; or if I did not notice his still more serious mistake about reddening the blood with a hot bath in the inflammatory stage of the climate fever, it was merely because I wished not to injure him, by pointing to what I considered as his errors, but rather to do him good; for, whoever the author of this successful practice may be, I believe that the run of success which they have had in Trinidad has no parallel in the army practice in any other sickly island in the West Indies; and as the merit of having *adopted* this practice was due to Mr. Greatrex, I thought I might serve one who appeared to me, when I saw him, a very interesting young man, by directing the attention of the army medical board to this circumstance. Such was my conduct, these were my motives, and, God knows, I have had my reward.

' In a letter written by Mr. Greatrex, *apparently in answer to a letter of Dr. Johnson*, dated July 24th, 1831, he states, " I have withheld, as you have seen by Dr. Hacket's letter, all authority for the publication of a *part* of my letter. Still Dr. Stevens has published a garbled extract from it." Now, whatever Mr. Greatrex may have said in his note to Dr. Hacket, or in his letter to Dr. Johnson, I do most solemnly declare, that I have never received any note, or any communication from Mr. Greatrex, except the document which Dr. Johnson has published, and which, I may here observe, is incorrectly printed in his journal.

' Now mark this. The Trinidad army practitioners admit, even by their own confession, every syllable to be true which I have stated in the paper to which they refer. They admit that their practice was such as I had described. They admit, also, that my statement was correct with respect to the result. They admit all that *I have said to be true,* and yet in the same document, and at the same time, they accuse me of having made *pointed assertions* and *misrepresentations,* which, I do say, *I did not make.*

'I am afraid that Dr. Hacket has been most miserably deceived by some of his correspondents *in this country;* but, at all events, it was the imperious duty of that gentleman, before he had ventured to make such an attack, first to have proved that I had in reality made even *one assertion* that was not correct, or drawn even one solitary conclusion which I had no right to have drawn. Had he done this, he would then, at all events, have been right in his premises, however wrong he might still be with respect to his conclusions; but the fact is, that he is wrong in both: for I do deny that I either made one assertion which I had no right to make, or drew even one conclusion, which I had no right to draw; and if I have, let them point out where it is, for I cannot find those pointed assertions which he says are not true, in my own paper, neither are they pointed out, much less proved, in his,—it is only on the strength of misstatements, which exist merely in their own imaginations, if they do in reality exist even there,—it is only on *assertions* of their own fabrication, that they have founded the whole of their attack. It is only on their own idle assumptions, which they are pleased to call my assertions—it is merely this imaginary or baseless cause of complaint which they themselves have conjured up, which they consider as quite sufficient for accusing me of misrepresentations and want of candour.

'It is not the statement which *I have made* that they attempt to deny. It is only conclusions which I did not draw, misrepresentations which I did not make, and pointed assertions which *I had not made*, that Dr. Hacket has been pleased to contradict "in the most pointed and unqualified manner;" and no person has a better right to do this than himself; for all the assertions that are not correct, and the whole of the misrepresentations which have been made on this subject, are entirely of their own fabrication. It is they themselves, therefore, who have misstated facts, by asserting that I have made pointed assertions which I did not make, and that I had drawn conclusions which I did not draw, and then, after mistaking these their own idle assumptions for facts, they commence an attack upon false premises, and in conducting this, two of them at least, use language that would not be creditable even to a corporal—language which is certainly not either becoming or proper in deciding on the treatment of an awful disease, and which would not have been at all necessary, if they had merely been under the influence of the warm glow of truth, in place of an evil passion, which Dr. Johnson candidly admits to have been the principal reason why he attacked me, and that too without the slightest provocation on my part.

'Dr. Johnson states, that for the honour of the profession he had made up his mind not to publish the Trinidad documents; " but," says he, " my attention being roused by the announcement of Dr. O'Shaughnessy's paper, and more especially by the very strong terms in which Dr. Stevens's discoveries were spoken of in a contemporary, in which it is stated that Dr. Prout considered these discoveries as of the last importance to mankind, I felt that it would be culpable, if not criminal in me, to withhold from the profession the counter-statements of the army medical officers of Trinidad, from which it appears that *soda* had nothing whatever to do with the *success* of the treatment in the above island." Now, if Dr. Johnson cannot bring forward any better reason than this for such an attack, he had better, perhaps, have kept it to himself, particularly if he has no other documents than those which he has hitherto produced, to prove the serious charges which he made against me in the Westminster Society, and which, I must say, were most untrue. So much for the manner in which Dr. Johnson maintains the honour and respectability of the medical profession.

' As I have said that Dr. Johnson has been guilty of wilfully endeavouring to mislead the public with misrepresentations about soda and sodafied blood, it may be but fair to show, by his own evidence, that this is the case; I therefore beg leave to refer you to his review of my paper on the blood, in which you will find the following extracts, contained in his number for June, 1830.

‘ “ At a late meeting of the College of Physicians, a paper on the above subject was read by Dr. *Steevens*, who has, for many years, been a practising physician in the West Indies. As the views which Dr. Steevens entertains are novel, and the means which he proposes for combating a dreadful scourge of the human race are *simple and practicable*, we have endeavoured to collect the substance of the writer's observations as accurately as possible, to lay before our readers.

‘ “ Our author observes, that one common property of neutral salts is that of giving a rich arterial colour to venous blood. This property is common to them all, and the degree to which they possess it is, perhaps, the best test of their purity as saline agents. To ascertain the effects of different agents on the blood, he made a number of experiments, in which it was observed:—

‘ “ 1st. That all the acids give a dark colour to healthy blood, and in proportion to their strength, change it from red to black, as certainly as they change vegetable colours from blue to red. Even the vegetable acids so completely blackened the blood, that the addition of a little water converted the whole into a fluid exactly resembling the black vomit. *Secondly, the pure alkalies have a similar effect with the acids, in changing the blood from red to black, though not in the same degree. Thirdly*, the neutral salts immediately change the venous blood from a dark Modena red to a bright arterial colour.

Even those salts that contain an excess of alkali, the sub-carbonate of soda for example, immediately give to venous blood a beautiful bright arterial colour. The effects of these experiments are best seen when made on healthy blood. The agents ought first to be dissolved in a little soft water, and then well mixed with the warm blood before it begins to coagulate.

'" 4thly. When the neutral salts are mixed with the dark and dissolved blood that had been taken from the hearts of those who had died of yellow fever, even this black and dissolved fluid was instantly converted from a black to a bright arterial colour.

'" The nature of this paper (*said* Dr. S.) prevents me from entering minutely on the important effects which this saline impregnation produces in the vital fluid; but in a work, which will soon be published, I shall endeavour to prove, first, *that the blood owes its red colour to this saline impregnation.* Black appears to be the natural colour of the colouring matter; for, when we take a clot of blood, and deprive it completely of its saline matter, by immersing it in fresh water, the colouring matter soon becomes so black, that even oxygen has no effect in changing its colour. But when we immerse this black clot in an artificial serum, made by dissolving some saline matter in water, the black clot in this clear fluid assumes, almost immediately, a beautiful bright arterial colour. Secondly, that to this saline impregnation the fibrin owes its fluidity, &c. &c. Thirdly, that the change of form which this saline matter undergoes, when the blood changes from arterial to venous, and from venous to arterial, alters its capacity for caloric, and gives it an influence in supporting the temperature of the system. *The saline impregnation also adds to the stimulating quality of the blood,* and assists, even in a high temperature, in adding to its powers of self-preservation.

'" As we have no doubt that Dr. Stevens will pursue this interesting inquiry farther, and lay the results of his observations before the profession in a more extended form, we shall abstain from any comments on the present occasion. We have laid a very full and faithful account of the paper before our readers, and leave them, for the present, to draw their own conclusions."

' As I trust that many of your readers will now feel an interest in this subject, even though divested of everything like unbecoming personalities, and as most of them have not seen the West India fevers, it may be proper for me to make some observations on the nature and treatment of these diseases, but, above all, to point out the *fatal error* in the old practice, which has been, as I believe, the true cause of one half of the mortality in the West India fevers, and, perhaps, even a great deal more.

'This I will do as early as possible, and lay it before the public as soon as I can.

'I am, Sir,
'Your obedient servant,
'W. STEVENS, M.D.'

'Albany-Street, Jan. 9, 1832.'

Since the publication of the above paper, I am indebted to the distinguished individuals at present at the head of the Army Medical Board, for a document, by which it appears, that from the 1st of June, 1829, to the 1st of March, 1830, there were exactly six hundred fever cases admitted into the Military Hospital at Trinidad. Mr. Greatrex states, that from the middle of August, 1828, to the 12th of May, 1829, the saline treatment had been tried in three hundred and forty cases, or thereabouts; and from the 12th of May, 1829, to the 1st of June, in the same year, there were about seventy fever patients admitted; thus making in all about one thousand and ten cases of the various West India fevers, in which the saline treatment had been tried; and out of this number there had been about eleven deaths: consequently, from the moment that they had commenced the saline treatment, in the middle of August, 1828, to the 4th of April, 1830 *, the mortality from fever had been

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* This is the date of the latest letter which I have from Trinidad.

reduced to about one per cent. This result is, indeed, very different from that which they had obtained, so long as they attended merely to the solids, and neglected entirely the vitiated condition of the vital current which exists so invariably in every one of the various West India fevers.

Were it at all necessary, more evidence might be produced upon this subject; but I trust that I have said enough to convince any impartial reader, that the following inferences may be justly drawn from the facts which have been stated:—

First, That the army physicians in Trinidad were, like the other practitioners in the sickly West India islands, most miserably unsuccessful in fever, so long as they looked merely to the solids, and trusted to the common remedies which were then so much in general use.

Second, That in the month of August, 1828, they did adopt a new practice, in which they attended first to the solids, and then to the vitiated condition of the blood; or, in other words, after having reduced the excitement by the lancet, &c., they then had recourse, not to calomel or sugar of lead, but trusted the cure to the energetic non-purgative alkaline salts.

Third, That from the moment they commenced this practice in fever, there was a sudden cessation of the mortality. I may also add, that I assured Mr. Greatrex, beforehand, that if he would give the saline treatment a fair trial, he would find it infinitely more successful than any other. We have now seen, by his own confession, that he did try it, and the result has verified the predictions. This, however, is only what might have been expected from what had previously already occurred in the island of St. Thomas.

CHOLERA.

When this disease, which is generally believed to be of Asiatic origin, burst out at a late period from its usual boundaries, and extended its ravages towards the West, it was generally believed to be less amenable to treatment than it really is, consequently it spread for a time an alarm all over Europe. Almost every government was in arms against it; and the whole medical talent of the day appeared to be concentrated for the purpose of investigating its nature, and preventing the mortality from this new but destructive pestilence.

It is well known, that those practitioners in this country who had formerly seen the Cholera in India remained, in general, most obstinately pertinacious of their former opinions, both with respect to its nature and treatment; whilst others, who had not yet bound themselves to any theory, and were well aware that nothing had been done in the East towards lessening the mortality, were eager in their endeavours to find out some more fortunate method of treatment than those which had hitherto been used with so little success in other countries.

It would be, however, but a loss of time to enumerate the various remedies which were proposed, tried, and found to be useless. When the Cholera first appeared on the isolated shores of this island, white wine whey with spice, hot brandy and water, cajeput oil, peppermint, laudanum, &c., were officially recommended to the public by one Board of Health; whilst another pressed into the service all the remedies which had ever been thought of, and recommended the whole to the profession in a confused mass. They approved of red-hot irons to the spine, and bleeding, together with the internal use of opium and emetics, James's powders, calomel, Cayenne pepper, chalk and brandy, ice, quinine, *salts*, *acids*, &c. &c. But, notwithstanding all this, the mortality continued, as might naturally have been expected; for such remedies, so empirically used, cannot even mitigate the symptoms, much less cure the disease [*];—and, as a general rule, I firmly believe that there would have been fewer deaths if the patients had been kept in a warm room, allowed plenty of cold water, and then left to the care of a good nurse. There were, however, some exceptions to this rule; and from what I have now seen, my conviction is, that when Cholera is taken in time, and properly treated, it is, in the majority of cases, almost as easily cured as either the common typhus or the marsh fever.

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[*] These observations apply only to the *treatment* recommended by the boards of health; in some other respects the conduct of the present board appears to me to have been highly judicious; and if London has hitherto been saved from the awful scenes which have occurred in Paris, &c., it has been in no mean degree owing to the active and proper measures that were used to prevent it. I fear, however, that the danger is not yet passed; and from what I have seen lately, there is reason to dread that this metropolis is at this moment 'slumbering on a volcano of pestilence.'

The facts which I had previously stated relative to the effects of salts on the blood, appeared for a time to have been almost forgotten ; but, in proportion as the danger drew near, they were again recurred to and warmly recommended by some, as at least worthy of attention in the treatment of Cholera.

When it had been fairly proved on the continent that the practice which had been used in India was of no value, Sir Astley Cooper, Dr. Prout, Dr. Elliotson, Dr. James Wilson, Mr. Travers, and some other talented individuals, who consider their profession as something more than a mere trade, openly expressed their opinions in favour of the saline treatment; and that, too, in a manner which renders it impossible for me to find words to express how grateful I feel to these and some other scientific individuals, who had the good sense to perceive that the diseased condition of the blood had been too much overlooked, not merely in cholera, but in other diseases.

Previous to the appearance of cholera in *this* country, the following paper appeared in the Medical Gazette for September 3rd, 1831 :—

' *On the State of the Blood, and Effect of Saline Medicines, in Malignant Diseases.*

' We have been informed, on the authority of

Dr. Harder, a physician to the court of St. Petersburgh, who accompanied the Grand Duchess Helena to England, that his countryman, Dr. Jachnichen, of Moscow, has demonstrated "that healthy blood contains a notable quantity of free acetic acid, which, as well as the natural portion of its serous fluid, is in a striking proportion lost in the blood of cholera patients; but that these substances are to be regained in nearly their right proportions in the fluids inundating the primæ viæ, in cholera, and voided by the vomitings and alvine evacuations in that disorder." We think it due to our Russian correspondent to make this announcement, the rather as it is intended to correct what he regards as an inaccuracy on our part, though we are free to confess that we are by no means convinced even now of the error lying with us. We cannot understand how acetic acid can remain *free* in a solution which contains soda; neither, as the addition of acids blackens the blood, does it seem probable that the removal of such agents should also have the effect of rendering that fluid more dark. It is not our intention, however, to enter farther upon a question, for the satisfactory discussion of which we lack materials; but we shall take the opportunity it affords of bringing before our readers the statements of Dr. Stevens, whose views with regard to the state of the blood in malignant diseases appear to us to merit more attention than they have received, and which are, to a certain extent, though indirectly, strengthened by some of the phenomena which the cholera has presented in the north of Europe.

'In our number before last we published a letter from Dr. Barry, in which he says—"Two physicians (Germans), Ysenbeck and Brailow, stated publicly and firmly yesterday, in my presence, at the Medical Council, that during the preceding eleven days they had treated, at the Custom House Hospital, thirty cholera patients, *of whom they had not lost one.* They give two table spoonfuls of common table salt, in six ounces of hot water, at once, *and one table spoonful of a similar mixture cold every hour afterwards.* They always begin by bleeding." A similar treatment has also been tried with success at Warsaw, by Mr. Searle;—and most of our readers will recollect, that this is neither more nor less than the practice which has been so warmly recommended by Dr. Stevens, not only in yellow fever, but also (reasoning from analogy) in the cure of all other malignant diseases; and after reprinting the first paragraph from the paper which had been read at the College of Physicians, the editor observes:—Now Dr. Stevens holds the saline materials of the blood to be the cause of its red colour, and certain other properties essential to life. He does not, indeed, identify the saline with the colouring matter; on the contrary, he considers the latter *as a mere animal dye, which is naturally black, but which possesses the peculiar property of striking a red colour with a solution of the neutral salts.* He also considers th saline ingredients in the blood as the cause of its fluidity, on the assumed ground that the fibrin and the albumen are naturally solid, and that the tendency to this condition is counteracted by the saline impregnation.

'"Nature," says Dr. Stevens, "does nothing in vain; and all the analyses of the blood have proved that, in health, it invariably contains a given proportion of saline matter. This is not accidental; for it is as essential, and exists as invariably in healthy blood, as either the fibrin, the albumen, or the colouring matter. Arterial must evidently contain a larger proportion, or at least a stronger saline matter, than venous blood; for all the solids, and most of the secretions, derive their saline matter from arterial blood. But the serum of even the venous blood which is left contains a proportion of thirteen ounces to the thousand of these salts, independently of what is lost by evaporation, &c. We well know how active these salts are as chemical agents; and these agents are so constantly found in the blood, and in healthy blood their proportion is so exact, that we are forced to believe that they are placed there for some important use. But the importance of this saline impregnation has been almost entirely overlooked, from the great attention that has been paid, both by physicians and philosophers, to the much less important colouring matter."

'Of all the ingredients, Dr. Stevens appears to consider the saline matter of the blood as by far the most essential to its healthy state. Any of the other ingredients may be diminished, and still the vital fluid will perform its functions ; but when the saline impregnation is lost, or greatly lessened, as he states it to be in malignant diseases, the blood becomes black, goes fast to decay—a change which is soon followed by death. "On examining the blood soon after death of those who had died of the yellow fever, the colour in the whole mass of blood, both in the arteries and veins, was completely changed from its natural scarlet, or Modena red, to a dark black. I have frequently filled one glass with the black fluid taken from the heart, and another with the black vomit taken from the stomach. They were both so unlike the blood of health, and resembled each other so completely, that it was almost impossible to distinguish the one from the other."

'The oxygen of the air had no effect whatever in reddening this dark fluid; but on adding a small quantity of any of the neutral salts, even to this black blood, the red colour was immediately restored; and Dr. Stevens believes that certain saline agents have a specific effect, when administered during life, in remedying that diseased state of the blood which is uniformly met with in this, and probably in all other malignant diseases.

'It appears that Dr. Stevens first commenced this practice with a solution containing two parts of muriate of soda and one of nitrate of potass. An objection to this mixture was, that when given in large doses it was apt to disagree with the stomach; and finding that other saline agents possessed, in an equal degree, the same property of remedying the diseased state of the blood, these were occasionally employed; the carbonate of soda, for example, was preferred, particularly when there were any signs of acidity in the stomach, and the tartarised soda was generally given when a purgative was required. Under this practice, the result is stated to have been incomparably more successful than it was under the old treatment.

'The resemblance between cholera and certain malignant forms of fever, both as to their phenomena, and, if some of the latest accounts be correct, likewise as to the treatment which proves most successful, is very striking. Even in those cases in which there is no excitement, and of course no increased action to be reduced, the removal of a small quantity of blood is equally beneficial in both, by relieving the overcharged heart, and enabling it to circulate with more ease that which is left. But in the malignant fevers of the West Indies, much more commonly than in cholera, there is great increased action, and the lancet is then used freely. As early as possible after the first bleeding, the patients are freely evacuated by means of some active purgative. As soon as the excitement is sufficiently reduced by these means, which it generally is in less than twenty-four hours, the use of the saline mixture is commenced. This method of treatment, which Dr. Stevens has the merit of having first proposed, is gaining ground in the West Indies, &c. &c.

'It appears certain that the mortality from fever has been lessened in those islands of the West Indies in which this treatment has been fairly adopted; and it seems to us, that analogical reasoning, as well as the statements of Dr. Barry and Mr. Searle, would fully justify a trial of the same method in cholera. This last is, indeed, so virulent a pestilence, and so many perish in the first stage of collapse, that no treatment which human ingenuity can devise will probably do more than rob it of a certain portion of its mortality; but should even this limited benefit be obtained by saline medicines, exhibited on the principle of remedying the morbid condition of the blood connected with its blackness, it will be chiefly owing to the statements of Dr. Stevens on an analogous subject; for though his paper, as we have said, has never been published, yet our account of it has made his doctrines extensively known, and will thus lead to their confirmation if true, or their rejection if false.

'*Cause of the Red Colour of the Blood.*

'As connected with the above subject, we may lay before our readers the following communication, extracted from an American journal. It is a letter from Dr. Sewall, the Professor of Anatomy and Physiology in the University of Washington, which we find published in the Boston Journal for December last:—

'"The cause of the red colour of the blood has long been a subject of keen discussion among physiologists, and nothing has hitherto appeared upon the subject at all satisfactory, and against which powerful objections could not be brought.

'" Dr. Stevens, an eminent physician of St. Thomas, has instituted an experimental inquiry into this subject, which has led to some novel and interesting results. From his experiments it appears—

'" 1st. *That the blood owes its red colour entirely to the presence of the saline matter which is invariably found to exist in it, while in a healthy state.*

'" 2dly. *That the dark colour of venous blood arises from the presence of carbonic acid, which, like every other acid, turns the blood black.*

'" 3dly. *That the oxygen of the atmosphere can only affect the colour of the blood inasmuch as it possesses a powerful affinity for carbonic acid, which it takes from the blood by attracting it through the delicate membrane that lines the bronchial vessels and air-cells of the lungs.*

'" 4thly. *That the removal of the carbonic acid from the blood by the action of oxygen does not produce a change in its colour, unless there be saline matter actually present, to impart to it the arterial tint the moment the carbonic acid is removed.*

"' 5thly. *That acids, alkalies, electricity, and every thing which destroys the neutrality of saline matter, gives to the blood a dark colour.*

'" Whatever practical inferences or change in the treatment of diseases these experiments may lead us to, the idea that the red colour of the blood is owing to the saline matter which it contains, is entirely new; and no one can deny to Dr. Stevens the merit of having been the first discoverer of this interesting fact. He is still prosecuting his inquiries; and his researches upon this and other subjects connected with it promise much to the profession. They will soon be laid before the public in detail. I have had the pleasure of witnessing a number of Dr. Stevens's experiments, as performed by his own hand, upon the blood; and so far as I have had an opportunity to examine them, they have been performed with great care and accuracy, and were entirely satisfactory."

'We are aware that Dr. Stevens himself is about to publish on this subject; yet, as there are some parts of Dr. Sewall's letter which we do not clearly comprehend, and as every thing relating to the blood and the treatment of malignant diseases possesses great interest at this moment, we have applied to that gentleman for information on certain points: should we succeed in obtaining this, we shall lay it before our readers in another number.'

In answer to the above, the following communication was immediately sent to the Editor of the Gazette.

ON THE TREATMENT OF MALIGNANT DISEASES.

'*To the Editor of the London Medical Gazette.*

'SIR,

'As I have never seen even one case of the Indian cholera, of course I can only judge of the treatment of that disease by reasoning from analogy, betwixt this and other malignant fevers which I have actually seen; but probably I was not far from the truth when I stated, that the practice which I had found so useful in the malignant fevers of the western world, would be equally successful in the treatment of all other forms of malignant disease; and perhaps, also, after this treatment has been fairly tried, the outline of the practice in all malignant diseases will ultimately be nearly the same. I can now add, that the same treatment, which I have recommended in the yellow fever, &c. has been most extensively used, and with equal success, in the treatment of those malignant forms of the marsh fever which were formerly so fatal in the Genesee country, on the southern shore of Lake Ontario. This country is so flat that the Erie canal runs through it for upwards of seventy miles without even one lock. From being so flat, it is full of marshes; and during the hot months, there are many of the districts in that territory nearly as sickly, and, until lately, nearly as fatal, as the marsh fevers of Sierra Leone. The result, however, is now very different, particularly at Rochester, and other places in that country, where the alkaline carbonates, &c. are now used in place of the calomel, or the mere purgative and bark treatment, which were formerly employed in these localities with so little success.

'The sickness of the stomach which is so generally met with in the commencement of all those fevers that are produced by the specific aërial poisons, is probably the effect of the poison itself, which is thrown out of the circulation, and causes irritation in the gastric organs, in the same way that tartarized antimony produces nausea and vomiting when we inject a small portion of that agent into a vein: when proper remedies are used, that sickness at the stomach which begins with the disease soon passes away; but the peculiar irritation in the gastric organs which comes on at a later period, and which is often so distressing in the last stage, is evidently in these fevers produced in a great measure by an excess of acidity in the stomach. This may perhaps arise from the decomposition of the saline ingredients of the blood by the nervous or electric fluid which appears to exist in excess in all fevers, but particularly in those of a malignant character*. After the decomposition of the saline matter, the acids of the salts may be attracted into the gastric organs, where they exist in excess, and act as a source of intense irritation. This, however, is in part theory; but there is no question of the fact, that there is in all the malignant fevers of the new world, particularly in the last stage of these diseases, an excess of acids in the alimentary canal, which extends from the very tip of the tongue to the verge of the anus. When we apply at this period of the disease a piece of litmus paper to the foul or red irritable tongue, the test is reddened almost instantly; and when we apply the same paper to the fluids ejected from the stomach, it is reddened almost as suddenly as if it had been dipped in a pure acid. In fact, even the matter of black vomit (which is merely an internal effusion of the black and dissolved blood) receives such an addition of fixed acid in the stomach that it effervesces freely with the alkaline carbonates.

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* This is most probably the case in the climate fever, in which there is no poison in the system.

'The excess of acid which produces the intense irritation in the stomach, is not the acetic, for even the matter of the black vomit has no sour smell. This excess of acid is probably derived from the saline matter of the blood: and as the muriate of soda is the principal saline ingredient in the blood; so I believe that the muriatic is in fever the acid which exists in the greatest excess in the stomach*; but whatever the source of this acidity may be, it is, as I have said, at this period of the disease the true source of the intense burning, and that local irritation, amounting in some cases even to inflammation, which is the real cause of the gastro-enterite of Broussais. This species, however, of the enterite cannot be cured either by gum-water, taken internally, or by leeches applied to the pit of the stomach. The irritation is produced by a chemical cause, and can only be removed by chemical means.

'It is at this period of the disease that the alkaline carbonates are of such infinite value: when we give, for example, the carbonate of soda, the fixed acids of the stomach are immediately neutralized by the alkali of the carbonate; a large quantity of carbonic acid is evolved by the mouth, and the irritation of the stomach disappears almost as fast as if it had been removed by a charm.

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* We are indebted to Dr. Prout for the discovery that the muriatic is the acid that is chiefly generated in the morbid conditions of this organ.

'By this treatment we not only remove that irritation and severe burning in the stomach which is so distressing to the patient, and even so destructive to the gastric organs, but we gain another point, which is, at this period of the disease, of still more importance than the mere removal of a local irritation. The fixed acids are, as I have said, immediately neutralized by the alkali of the carbonate. The muriate of soda, and the other natural salts of the blood, are instantly formed in the stomach itself. Now we know that these salts do enter the circulation; we know also that they mix with, and become a part of the circulating blood; we know that they change its properties and remedy its morbid condition; we know also that they add to the stimulating power of the circulating current, and enable the heart to keep up its action.

'In consequence of this addition of saline matter, the kidneys, and the other secreting organs, continue to perform their functions. The skin does not become yellow, nor the breath fetid; neither is the mortality one-twentieth part so great as it had been under the old modes of treatment. In fact, the successful results which have already followed the use of the above practice, prove that the saline remedies are the agents of all others the best that we yet know of, for the successful treatment of malignant diseases.

'When there is an excess of acid acting as the source of destructive irritation in the gastric organs, the treatment with the alkaline carbonates is decidedly the best; and those agents are as decidedly the worst, the effect of which is in direct opposition to that of the alkaline salts. When there is no excess of acid in the stomach, as sometimes occurs in fevers that are more mild, the carbonates enter the circulation unchanged; and we know that when they are mixed out of the body, even with the black blood taken from the heart of those who have died of the yellow fever, they redden its colour as much as the muriate of soda, or any of the other stronger salts. I have also stated, that all the acids blacken the colour of the blood so completely, that with the addition of a little water, even healthy arterial blood is immediately converted into a fluid exactly resembling the black vomit.

'The dark colour of the blood, which we observe in the beginning of pestilential fevers, is the effect of the poison on the vital fluid; but the blackness in the last stage of these diseases is produced *by the loss of the saline ingredients* *, which I can prove are beyond all question the true cause of the red colour of healthy blood. The mere fact that the blood has a dark colour in all the fevers which arise from poison has been long known, but the causes of this dark colour have been but ill understood. An attempt to redden the dark colour of the black blood in fever has been, with some practitioners, the chief object in the plan of cure; but ignorance of the real properties of the vital fluid has led to errors which have been even more fatal than those which now generally exist as the consequence of the doctrine of pure solidism. Acids redden the blue of vegetable colours; and these agents have been extensively used by a certain class of physicians to redden the blood in various diseases, on the supposition that they contain an excess of oxygen, which they would give over to the black blood, and thus redden its colour. The fact is, however, that though acids redden the vegetable colouring matter, they completely destroy the red colour of the blood; yet these are the very agents that in fever have been thrown so unmercifully into an organ, already burning from an excess of acid, on purpose, as they say, to redden and revivify the colour of the dark blood.

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* The above paper was written previous to the appearance of cholera in this country. The blood in this disease has since been analyzed by Dr. O'Shaughnessy, Dr. Turner, Dr. Thomson, of Glasgow, &c. They all agree in the fact, that there is a material

'I shall afterwards have occasion to bring forward some melancholy proofs of the fatal effects of the

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diminution of the saline matter. Dr. Thomson, however, makes the diminution to be less than any of the others; but the cause of this has been very ably pointed out by Dr. O'Shaughnessy in one of the late numbers of the Lancet.

acid treatment, and to show that, in some places, it has been already used to a fearful extent. The calomel, and some other modes of treatment, have done much mischief*, but the acids have been the agents, of all others, the most destructive in the treatment of the yellow fever, and other diseases that really possess a malignant character.

'It has been already stated, that when the blood is black from the loss of its saline ingredients, oxygen is not attracted into the circulation in the lungs after the removal of the carbonic acid; at least, if it be attracted at that period, even the strongest oxygen has no more effect in reddening the black blood than it has in reddening the black clot that has lost its saline matter, and of course its red colour from immersion in distilled water. Yet, though this practice has been already weighed in the balance and found wanting, we are annoyed almost daily by the recommendation of means for oxygenating the black blood. Oxygenating the blood, however, is of no use in such cases, for the blood can only be reddened by saline remedies. Calomel and antimony may fret the stomach, and add to the suffering of the patients. Acids and opium may and do darken and destroy the red colour of the blood; but when the red colour is lost, as in bad fever, it can only be restored by the use of those remedies which are, in reality, in its healthy state, the true cause of its redness.

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* The calomel practice, in cholera, had been tried at Warsaw, and found to be of no use, even before the arrival of Mr. Searle in that city. Notwithstanding this, he commenced with his favourite remedy; but, according to the latest accounts, he had been trying the saline treatment with the most marked success. The calomel practice appears to have completely failed, even in the hands of Mr. Searle, who is known to have been one of its warmest advocates. In one of the late numbers of the *Journal Universel et Hebdomadaire*, we find it stated in a letter from M. Londe, the President of the French Commission, now in Poland: that 'Les moyens principaux que l'on emploie ici contre le choléra sont: 1°, le calomélas à forte dose (*huit à vingt grains par heure, ou même par demi-heure*). Il est administré ici dans l'hôpital de Bagatelle, par M. S——, médecin anglais. Dans cet hôpital, la mortalité est effrayante.'

'It may easily be ascertained, by the litmus paper, whether there be or be not, in cholera, an excess of acid either in the blood or in the fluid ejected from the gastric organs. If there be an excess of acid, then the alkaline carbonates are the remedies, of all others, the most likely to be useful; if there be no excess of acid, then the mixture of muriate of soda and nitrate of potass may probably be preferred; and as all parties agree in admitting that, during the first stage of cholera, the blood is not only diseased, but black in colour and thick in its consistence, I am, therefore, inclined to believe that, *under all circumstances, the non-purgative saline medicines are the remedies, of all others, the most likely to be useful;* for they not merely redden the colour of the blood, but, by increasing the fluidity of its solid ingredients, and adding to its stimulating power, they will render the blood more fluid, and, of course, better fitted to serve the important functions which it is intended to perform in the living system.

‘ I will afterwards bring forward some very strong facts to prove, that the aërial poisons which act as the remote cause of the essential fevers, do not produce their effect by any direct impression on the nervous system; on the contrary, they appear, like the oxygen of the air, to be attracted into the circulation, and produce their effects on the solids of the system, entirely through the medium of the blood. The diseased state of the blood is the immediate cause of fever—the diseased action in the solids is merely the effect. I have seen cases in which there was no excitement from first to last, yet these very cases, in which the solids were not injured, even in the least, were of all others the most fatal.

‘ All the fevers from poison are generally preceded by a stage of torpor; for the first effect of the poisoned blood is to paralyze the heart, and indeed the whole of the vascular organs. The continuance of this cold stage is in proportion to the quantity or the virulence of the poison that has been taken into the system; but in all such cases, re-action is the road by which the animal economy marches to health, and the first duty of the physician is decidedly to bring on re-action, or fever, as speedily as he can. When this is effected, should the re-action run high, the excitement may be reduced by the use of the lancet, and the typhoid symptoms, which sometimes afterwards occur, may probably be prevented by the subsequent use of the carbonate of soda and other saline medicines, which we know do possess the power of preventing that black and dissolved state of the blood, which is, in reality, in fever, the true cause of the nervous as well as the other bad symptoms.

'The diffusible stimuli produce their effect, in some cases, by a direct and transitory impression on the nervous system; but, as already stated, the saline agents enter the circulation, mix with, and become a part of, the blood. The blood is the natural stimulus of the heart, and the active non-purgative saline medicines decidedly add to its stimulating power; these, when given early in cholera, and in active doses, will, by increasing the stimulating power of the vital fluid, enable it to act with more force on the vascular organs, and in this way rouse the patients from that cold fit, or stage of torpor, in which it appears they generally die.

'From what I have seen of their effects in other diseases, I have little doubt that, if the saline medicines be fairly tried, the mortality from cholera will be considerably less than it has hitherto been; but, to say the truth, *I do not anticipate much advantage from either the saline or any other remedies, or believe that they will be fairly tried, or generally successful,* so long as they are used by practitioners who believe that fever is a nervous impression, and who believe also that all our remedies in that disease act merely by sympathy, or some mysterious agency, on the nerves of the stomach.

'It is well known that many practitioners have long been in the habit of using the saline medicines, particularly as purgatives, in the treatment of fever; and many still continue their use, merely for the very substantial reason that they find them useful. The true reason, however, why these remedies are so decidedly superior to all others, in the treatment of this disease *, has not, I believe, been generally understood; and therefore these medicines are often combined with acids, calomel, or other adverse and powerful agents, which prevent the good effects that would otherwise have followed the judicious use of the active saline agents, when given on a steady principle, and used only at certain periods of the disease, where they can do no harm, and when there is almost a certainty of their doing good.

'I know it will be asked, why have the citric and other acids been successful in scurvy, where the blood is darker than it is in health? To this it may be answered, that the scurvy is not, like the cholera, or the yellow fever, a disease that causes death in a few hours, or a few days; and therefore medicines that may be used without causing immediate death in the one, cannot be used in the others with equal impunity. My own conviction is, however, that there is no one disease in the whole catalogue in which the profession has been so much misled as in the

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* I know one respectable practitioner in this country, who has been using the chlorate of potass for the last nine years with great advantage in the treatment of typhus.

very disease now under consideration. During a residence of twenty years in the West Indies, I have only seen one case of scurvy, and *that case was decidedly brought on by the excessive use of citric acid, which an American gentleman had been recommended to use as a preventive against the yellow fever.* His own conviction, as well as mine, was, that the scorbutic symptoms had been brought on by the acid. This was immediately laid aside, and, under the use of the carbonate of soda, he was completely cured in three weeks. To those, however, who are disposed to see the contrast betwixt the effects of the neutral salts and the citric acid, in the treatment of scurvy, I would recommend the perusal of Mr. Cameron's paper on this disease, which they will find in the Medico-Chirurgical Review, in one of the numbers, for 1829.

'It has long been, and I am sorry to observe still is, a common source of error, particularly in fever, to confound a similarity in certain symptoms with a sameness in kind. The sporadic cholera, which is occasionally met with during the hot months, both in this and in other countries, is evidently as totally different from the Indian cholera as east is from west. The one is a symptomatic affection, followed by a mere momentary excitement, arising in part from a severe local irritation in the gastric organs; the other, however, is a most malignant disease, produced by the existence of a specific and virulent poison in the system, which contaminates every drop of the blood, and excites diseased action in every solid of the body. Such being the fact, it is evident that remedies which are successful in the one, may be not only inert, but even actually injurious in the other.

'The Asiatic cholera is, as we well know, a most fatal disease, and will require both an active and judicious treatment to overcome the evil effects of the morbid poison; while the sporadic or plum cholera of this country may, I believe, in most cases, be almost entirely left to itself to work its own cure; and were it at all necessary, the cases which are now so numerous in most of the journals, might be faced by others, where the patients were obstinate, and refused to take any other remedy except a little warm brandy and water, which was given during the cold fit, on purpose to bring on re-action as speedily as possible. In one case, which I saw lately by accident, the symptoms, for the time, were quite as severe as those described in the various journals; yet, though the patient refused all remedies except warm brandy and water during the cold stage, he was just as well (perhaps even better) on the following day, than if he had taken 125 drops of the cajeput oil.

'There is one circumstance connected with the history of cholera which renders it a much more formidable disease in northern latitudes than either the yellow fever or the plague. The poison which produces the yellow fever requires a given degree of heat to enable it to exist in the atmosphere; and when the thermometer either rises or falls above or below a given range, the plague disappears. But the poison of cholera is not rendered inert by the first morning of frost, as is the case with the poison of the yellow fever in the United States of America; for it appears that this poison, like that of the small-pox, can produce its fatal effects almost as certainly in the middle of winter in Russia, as in the burning plains of the Torrid Zone. When once introduced, the contagious poisons possess the power of multiplying themselves; and, as the cholera poison acts in every temperature, it is more than probable that if it once finds its way into this country, it may remain here as a fatal scourge, not only to the present but to future generations. This, as well as the great mortality caused by the poison of cholera, imposes a solemn responsibility on those who are, or at least ought to be, the guardians of the public health.

'At present I have merely given a general outline, but the same subject will be considered hereafter more in detail. The above has been written in haste, and may probably contain more errors than one: should the treatment, however, which has been so useful in the malignant diseases of the New World, be found, even in the slightest degree, to lessen the sufferings or diminish the mortality of fever in the other divisions of the globe, I shall then be repaid for the dislike which I now feel in appearing before the profession as the advocate of doctrines so much in opposition to the common opinions of the present day. That this may be put down to its proper account, is the wish of, Sir,

'Your obedient servant,

'W. STEVENS, M.D.

'*September* 5, 1831.'

I may here observe, that when the paper was read at the College of Physicians, about fifty copies of it were hastily printed, and most of them were distributed chiefly amongst my friends in this country. Three or four copies of it were also sent to Paris, and the same number to some of my friends in Germany. The experiments and facts contained in that paper, as well as the reviews of it which had appeared in this country, were translated into the French and German journals, and circulated by these all over Europe. It is, therefore, not improbable, that it was this which led the two German physicians* to the use of the saline treatment in cholera. The muriate of soda was the remedy on which these gentlemen chiefly relied; and by way of having a theory of their own, they used it as an emetic, and gave it in the beginning in such doses that it produced vomiting, after which they used it in smaller quantities; and to this, in all probability, they were entirely indebted for their great success.

In 1831 these gentlemen had charge of the Custom House Hospital at St. Petersburgh, in which there were in all, during the epidemic, thirty cases of cholera; and of this number they lost three patients, and twenty-seven recovered. I mention this fact on the authority of Sir William Crichton: who states also, that, at the request of the Emperor, he had communicated this practice to the army physicians in Poland, by whom it had been found to be very advantageous*.

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* Namely, the two that are referred to in the letter of Dr. Barry.

About the same time, Mr. Searle tried this treatment at Warsaw. He used it in eight cases, and in every one of them he succeeded in bringing on reaction. Most of them, however, died afterwards,—not from cholera, but under circumstances of the most gross neglect on the part of their attendants. Mr. Searle at that period considered the muriate of soda as *a valuable remedy;* but when he had an opportunity afterwards of giving the saline treatment a fair trial at Berlin, in place of doing this—by which he would probably have done much good, and gained great credit to himself—he returned to his old calomel practice; but the destructive effects of this were so obvious, that the German physicians compelled him to discontinue its use in that capital.

As soon as it was generally known that the cholera had made its appearance in the north of England, I wrote to a physician in Sunderland, recommending him earnestly to give the non-purgative neutral salts a fair trial, in any cases where either himself or his friends might have an opportunity of using it. Soon afterwards I sent him a second letter; but from that day to this I have not received an answer to either the one or the other. His reasons for such conduct are best known to himself. He had previously pretended to be my friend, and, so far as I am aware of, I had never given him the slightest offence either in word or deed.

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* See the Supplementary Number of the Medical Gazette for January 7th, 1832.

When the cholera made its appearance in this metropolis, I did everything in my power to induce my medical friends who were in practice to try the saline treatment, and some of them did give it a trial in a few instances; but it was either in the most hopeless cases, or in the very last stage of the disease; consequently, the result made no very favourable impression. Others used the saline treatment in milder cases; but they combined it at the same time with opium, brandy, calomel, and other improper agents; consequently, the good that was gained by the one was lost by the others.

About this period, cases were published almost daily in the various journals, stating the successful result of the saline treatment; but still all this made no impression, either on the Board of Health, or on the generality of practitioners. Those who believed that cholera was produced either by a nervous impression, or a local inflammation, would not try the saline practice, because they could not see on what principle it could possibly do good. Some would not try it because it was too cold for the stomach;

and one philosopher of the right old breed, who appears to be ignorant of every improvement that has been made in the profession for the last fifty years, when he was told that in cholera there is a deficiency of saline matter in the blood, declared that it was all trash,—and even if the fact were true, he could believe it just as possible to make up for a want of the bile by throwing ox-gall into the stomach, as credit even for a moment that salts could be of any use by entering the circulation, and acting on the blood*. In short, almost every one that I met with had a theory or a practice of his own which he was determined to support, and was equally ready to throw cold water on everything like improvement that was proposed by another. There were, however, some brilliant exceptions to this rule.

In the beginning of April, I received a visit from Mr. Pout, a medical gentleman in Albany-street, who called to inform me that the cholera had broken out in the prison at Cold Bath Fields, and that he had been requested by Mr. Wakefield, the surgeon who had charge of the prison, to say that he would be glad to show me the cases; and from what he had heard of the saline treatment, he should be very willing to give it a trial—the more so, as he had now no longer any faith in the common remedies.

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* Such physicians as this are justly entitled to the eulogium which a certain practitioner gives to his son, in Molière's inimitable comedy of Le Malade Imaginaire—'Mais, sur toute chose, ce qui me plaît en lui, et en quoi il suit mon exemple, c'est qu'il s'attache aveuglément aux opinions de nos anciens, et que jamais il n'a voulu comprendre, ni écouter les raisons, et les expériences des prétendues découvertes de notre siècle, touchant la circulation du sang, et autres opinions de même farine.'

On the receipt of this message, I immediately went to the prison; and after some conversation with Mr. Wakefield on the subject, he not only agreed to adopt the saline treatment, but invited me to attend the cases along with him. He consented also that Mr. Crooke, a young medical gentleman who had lived with me for several years in the West Indies, should be allowed to remain constantly in the prison, to see that the medicines were faithfully administered, as well as to take notes of the cases.

The following is an outline of the practice which was pursued, not only in the prison, but everywhere else where I have had an opportunity of treating the disease.

First. The treatment was generally commenced with a Seidlitz powder, which was given with a view of lessening the gastric irritation, and partly for the purpose of removing the diseased secretions from the intestinal canal.

Secondly. When the stomach was irritable (which it generally was) a large sinapism was immediately applied to the epigastric region, and where the patients were cramped in the extremities, frictions were used with hot flannel. The pain produced by the spasms in the muscles were not only relieved by the frictions, but by this and the application of sinapisms to various parts of the body, the quantity of animal heat was increased, and this, I need scarcely say, is an object of great importance in the treatment of cholera.

Thirdly. A powder containing

Carbonate of Soda, ʒss.
Muriate of Soda, ℈j.
Chlorate of Potass, grs. vij.

was dissolved in half a tumbler of water, and given soon after the Seidlitz. In severe cases, the above powder was administered every half hour. In those that were less severe, it was used every hour, and in some malignant cases it was given every fifteen minutes. In short, it was given more or less frequently according to the circumstances of the case, and continued until the circulation was fairly restored; it was then given at longer intervals, and when the re-action was completely established, it was left off by degrees.

Fourthly. Where the stomach was irritable, the use of the above powder was occasionally suspended, and common effervescing mixtures, or small doses of the common soda powders, with an excess of the carbonates, were frequently used, until the irritation was lessened, and then the carbonate of soda with larger doses of the chlorate of potass were generally given without the addition of the muriate of soda, and frequently in such cases the chlorate of potass was given by itself, in doses containing ten grains each.

Fifthly. A solution of muriate of soda was also thrown up into the intestines, at as high a temperature as the patients could well bear this saline fluid.

Sixthly. In two very severe cases, which occurred out of the prison, the patients were put into a hot saline bath with evident advantage. It is well known, that a hot saline fluid is a better conductor of heat than fresh water at the same temperature; but, independent of this, a part of the saline ingredients may be absorbed from the skin, and the patients may also be benefited by respiring the hot saline vapour. It is but fair to state, however, that this means, which was evidently beneficial in the cases in which it was tried, was proposed by Mr. Marsden, one of the surgeons to the Free Hospital in Greville-street.

Seventhly. Seltzer water was allowed *ad libitum*, when the patients expressed a desire for something to drink. A strong infusion of green tea was also occasionally used, in severe cases, apparently with advantage.

Eighthly. It was considered essentially necessary to keep a large fire, both night and day, in every room where there was a patient with cholera. It is now well known that in by far the majority of cases, the collapse commences betwixt two o'clock in the morning and six A. M., or, in other words, at the period of the twenty-four hours when the atmosphere is coldest: from which it appears that external cold acts as an exciting cause to the state of asphixia. But independent of this, we have seen that the degree of force, with which oxygen can remove carbonic acid through the medium of a membrane, depends, in a great degree, on the temperature of the two fluids. Now, when the temperature of the blood is so very low, as it is during the state of collapse, and if the air which the patients then breathe be also cold, the small quantity of carbonic acid which exists in the black venous blood, will not be attracted by the cold air, and consequently this of itself may be one cause of the sudden death.

Ninthly. It is necessary to be very careful not to dismiss the patients as cured until they have been, at least, several days completely out of danger. Two of the cases which proved fatal in the prison, at Cold-Bath Fields, were lost from our not having been at that time sufficiently aware of the importance of this.

Tenthly. The patients ought not to be allowed to use one particle of solid or indigestible food, for at least five days after they have recovered from the state of collapse. We nearly lost more cases than one, from the too early use of solid indigestible food; and one woman, a nurse in the London Free Hospital in Greville-street, actually died from this cause, after having been considered as completely out of danger from a most violent attack of cholera accompanied with collapse.

Eleventhly. Those who put their patients under the saline treatment, ought to trust almost entirely to this; for if they use calomel, brandy, or other destructive agents at the same time, they will do little good; but above all, not one particle of opium ought to be given internally; for, from what I have seen, I consider this to be as fatal in cholera, as it is in the last stage of either the African typhus or the seasoning fever of the West Indies. Where the stomach, however, is extremely irritable, about twenty-five or thirty drops of laudanum, diffused in a little tepid water, may be injected with a small syringe into the rectum, not only with impunity but considerable advantage.

When the stomach is very irritable, small quantities of milk, with carbonate of soda, may be given occasionally; and when we use the saline powders in such cases, they ought to be dissolved in a very small portion of water.

When the case is exceedingly malignant, or where we are called in late in the disease, and find the patient already in collapse, we ought then to have recourse to the most active measures. An ounce of the muriate of soda, with half a drachm of the chlorate, or the muriate of potass, should be given immediately in cold water, and repeated, if necessary, every half hour, until the patient has taken about three doses of this strong solution. Should re-action be brought on by this, it may then be kept up by the common saline powders; but should this experiment fail, we may then, as a last resource, give the patient another chance for life, by injecting a saline fluid into the veins.

The ejections, and every other source of impurity, ought to be immediately removed from the room where the patients are; and the infected wards should be fumigated at least twice a day with gunpowder, and every particle of suspicious clothing, bedding, &c., should be boiled, for at least half an hour, in a strong solution of common soda.

Those who are recovering from the disease are liable to a relapse, and such cases are generally fatal; but from what I have seen, my belief is, that those who have completely recovered, after having had the cholera once, have an immunity from any future attack of this disease.

The above is an outline of the treatment and means which were used: the following is, I believe, a fair statement of the outline of the result:—

The three first cases which occurred in the prison were treated by Mr. Wakefield in the common way—with opium, brandy, the hot-air bath, &c.; but they all died after a very short illness. Almost immediately after this, another case was treated in a similar way by another practitioner, who had been sent for to the prison during the night, in consequence of Mr. Wakefield being unwell at the time. This gentleman was not then aware that any new practice had been adopted in the prison. He treated the patient *secundum artem*, with brandy, opium, and chalk; but the result was, that this patient was past all hopes of recovery before either Mr. Wakefield or myself saw him in the morning;—consequently, in the four cases that were treated in the prison in that way, there were four deaths and not one recovery under the common practice.

It may be proper to state, that previous to the beginning of April there were no bowel complaints in the prison, and the whole of the prisoners were then as healthy as they generally are at that season of the year. The first case that was reported to the Board of Health occurred on the 5th of April;—the saline treatment was commenced on the 8th. There were in all at that period about one thousand three hundred souls in the prison; and from the 8th of April to the cessation of the first epidemic, there were at least one hundred individuals who were evidently, more or less, under the influence of the poison.

In about fifty of the above cases, the patients were attacked with a bowel complaint, and most of them had, more or less, irritation at the stomach. The fluids that were ejected were generally deficient in bile; and the bowel complaint was attended with the following peculiarities:—

First. The inclination to go to the night-chair came on more suddenly than it generally does in cases of common diarrhœa.

Secondly. The ejections were less bilious than in common diarrhœa; and opium, chalk, astringents, &c., which are generally useful in cases of common bowel complaints, were of no use in checking the diarrhœa, which occurs when the patients are under the influence of the cholera poison. These remedies were chiefly used in cases which occurred out of the prison; but they evidently had no effect in checking the specific ejections which are produced by the cholera poison; and this I presume was the cause of the diarrhœa which occurred in the fifty cases in the prison to which I refer. The whole of these were immediately put under the saline treatment, and this appeared to give an immediate check to the disease; and I believe it was owing to the saline remedies, as well as to the circumstance of their being constantly kept in a warm room, and well taken care of, that not one of these cases ended in collapse: consequently, though they were constantly breathing in an atmosphere completely impregnated with the poison, yet not one of them was lost.

There were also about thirty-one similar cases, in which the above symptoms were still more distinctly marked, and in many of them the bowel complaint was more or less accompanied with cramps. These were all treated in the same way with the non-purgative salts, and in three, four, or five days, every one of them were sent from the observation ward, as we believed at the time, completely out of danger. I am sorry, however, to be obliged to add, that two of these cases which had been unfortunately dismissed too soon, and sent back as cured to the cold wards of the prison, were attacked with collapse during the night; and before they could again be put under the saline treatment, their stomachs were so irritable that they could scarcely retain even a tea-spoonful of water; and both these cases proved fatal in a very short period from the commencement of the collapse.

In addition to the eighty-one individuals already referred to, we had about nineteen cases in the prison, where the patients were either attacked with the disease, and got into a state of asphyxia in the cold wards of the prison during the night, or where the stomach was so irritable in the first stage that it could not retain the stronger salts. In almost every one of those cases the disease assumed a most malignant character. These were all treated with the energetic non-purgative saline remedies; and in the nineteen malignant cases to which I now refer, we had eighteen recoveries, and only one death; consequently, the total number of patients, who were all evidently under the influence of the cholera poison, was about one hundred, yet in those cases where we trusted almost entirely to the saline practice, we had only three deaths, and ninety-seven recoveries.

In corroboration of the above statement, I will insert here the following letter from Mr. Wakefield, which was published in the Medical Gazette for April 28, 1832.

['In further illustration of the treatment which has been adopted in the cases of cholera which have occurred at Cold-Bath Fields, we insert the following communication from Mr. Wakefield, the intelligent and highly respectable practitioner who has the medical charge of the prisoners.']—*Editor of the Gazette.*

'Lansdown-Place, Brunswick-Square,
April 25, 1832.

'SIR—So much has already been written on the subject of cholera, that I should not now appear before the public, but from a conviction that the facts which I am about to state, if generally known and properly authenticated, (which they can easily be,) must be useful to those of the profession who in future may be called upon to treat this new, but most malignant disease.

'The first case which I saw occurred on the 5th of this month, in the prison at Cold-Bath Fields. Three others quickly followed, and were immediately put under the common treatment: these four patients died, after a short illness, with all the symptoms of cholera distinctly marked.

'Soon after the commencement of the disease, a number of the prisoners were attacked with marked symptoms of derangement in the gastric organs; and as all of these cases occurred in the infected part of the prison, it is more than probable from this, as well as the general appearance of the patients, that the diarrhœa with which they were attacked, was the effect of the poison which produces cholera. From having seen similar cases in the commencement transformed rapidly into a state of collapse, my conviction is that every one of those patients were more or less in serious danger; and I believe also that, had they either been left to themselves or improperly treated, the majority of these cases would have run into a state of collapse, perhaps in a few hours; indeed I have little doubt that the one-half of them would have been lost under the practice which is generally adopted in the treatment of this disease.

'Independently of the numerous cases where the individuals were labouring under the premonitory symptoms, I have now had twenty-five cases of decided cholera, where the patients were in a state of collapse; and in justice to Dr. Stevens, who suggested the use of the saline remedies, as well as from a sense of what I owe to the public, I conceive it my duty to state, that after having seen both the old and the new treatment fairly put to the test, I am fully convinced that the saline practice is not only the most scientific, but decidedly the most successful that has yet been adopted for the cure of cholera; and from what I have seen, my conviction is, that if this treatment be fairly and extensively tried, the mortality from cholera will be greatly diminished. When used at an early period, it either prevents or arrests the progress of the fatal symptoms; and even where this treatment is not used until a later period of the disease, its effects are distinctly marked; and I may safely say that I have seen several most malignant cases recover from the state of collapse under the saline treatment, where the patients, I doubt not, would have died under any other practice.

'We have now upwards of twelve hundred persons in this prison; and from the commencement of the disease up to this date, there have been nearly one hundred cases where individuals have been more or less evidently labouring under the influence of the cholera poison. Twenty-five of these assumed the malignant character of the disease, having the majority of the symptoms described in the printed document issued by Dr. Macann. Four of the first cases, as before observed, were treated in the common way, and every one of them died. All the others, however, were immediately put under the use of the saline practice, as recommended by Dr. Stevens, and out of the whole number who have been thus treated, we have only had three deaths from cholera, and two of these were cases of relapse. I may state also, that within the last few days I have had one most malignant case in the New Prison at Clerkenwell, where the patient was in a state of complete collapse before I saw him. His extremities were cold; his pulse at the wrist was entirely gone; he had the cholera voice, and his tongue was icy cold. This man, like those in the other prison was immediately put under the saline treatment with the happiest effects, and I consider him now in a state of convalescence.

'I am, sir,
Your obedient Servant,
'H. Wakefield.'

It has been observed, lately, by individuals who are still anxious to cling to their former opinions, that the above cases occurred at a period when the disease was on the decline in this metropolis. These gentlemen forget, however, that London is not a village, and that though the disease was then decreasing in Southwark, Rotherhithe, &c., where it first commenced, yet at that period it was only beginning in that part of the metropolis where the prison is situated. There is also one most important fact which the said individuals forget to notice, namely, that almost every one of those patients, either in or out of the prison, died, who were treated in the same quarter and at the same time with the remedies recommended by the Central Board of Health; whilst the fact is equally certain, that almost every one of those cases recovered which was treated, either by myself or others, with the non-purgative alkaline salts.

There were other individuals, even lately, who had so little respect for their own reputation as to deny that cholera ever existed in London; and those are equally in error who believe, at present, that this disease has ceased to exist, merely because the Board of Health may not think proper to publish an account of the cases; but the truth is, that in many parts of London the cases, at this moment, are as numerous, and just as virulent, or perhaps even more so, than those that occurred at an earlier period.

In the first irruption of cholera which occurred in the prison of Cold-Bath Fields, the disease was confined entirely to the males. It commenced in the beginning of April, and the last case was dismissed cured on the 30th of the same month. From this period up to the 3d of June, there were no new cases; but on that day it broke out a second time. In this instance it commenced amongst the females, and soon spread almost all over the whole establishment, and is now at this moment much more virulent, and I am sorry to add, more fatal, than it has ever been at any former period. In the first case that occurred the woman was attacked on the night of the 3d, and died on the 5th. Her sister, who attended her, was next taken ill, but recovered under the saline treatment.

Soon after the commencement of this second irruption, I called at the prison, and there were then four cases. These were under the saline treatment, and as they were all doing well, I did not return. On the 21st of June, however, I received a note from Mr. Wakefield, requesting me to meet him at the prison as soon as possible. When I went there, I found about twenty patients with cholera, and out of this number five were actually dying. There was one obvious cause for this, which I do not feel myself at liberty to point out,—suffice it to say, that it originated from either a mistake or neglect on the part of the nurses who administered the medicines.

A saline fluid, similar to that which had been used at Leith, was injected in two cases, into the veins;

but the one died almost immediately, and the other though he rallied for a time yet he also ultimately died *.

From the commencement of this second irruption there have been in all, about eighty-one cases: many of these have been of the most malignant description †. Out of this number there have been thirteen deaths, and the other sixty-eight have either recovered or are now apparently nearly out of danger; but new cases are brought into the infirmary almost every hour. They are all of them however now under the most energetic treatment, and I sincerely trust that the mortality of the disease will be arrested in its progress.

When the cholera was first raging in the prison at Cold-Bath Fields, the disease broke out about the same time amongst a colony of itinerant Italians who resided in the neighbourhood. The first cases were put under the care of a physician, who had charge of a cholera hospital in that part of London. These patients were first bled, and then most scientifically treated with opium and brandy, but the result was such that the other Italians who were taken ill about

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* Should I have occasion to try this experiment again, I shall certainly add a portion of the chlorate of potass to the other ingredients. The muriate of soda answers very well for a time, but it is too apt to run off by the bowels; and then the blood is swamped by the large quantity of water.

† The most awfully virulent cases which I have ever seen, occurred in some individuals who had been employed to scour the blankets, &c. from the different wards.

the same time, refused to be treated by the cholera physician; and fortunately for themselves sent for Mr. Whitmore, an intelligent practitioner, who lives in that neighbourhood. This gentleman had seen the effects of the saline treatment in the prison, and afterward trusted entirely to this, in every decided case of cholera which he attended. Since then he has had in his own private practice about thirty cholera patients, chiefly amongst the Italians, and out of this number he has lost only two cases, and saved about twenty-eight; and this surely is no common occurrence, particularly amongst the poorer classes, where we do not always see them early, and where we are not certain either that the medicines which we prescribe are properly administered, or that they are not occasionally used at the same time with other improper agents which counteract the beneficial effects of the alkaline salts.

The first case which Mr. Whitmore lost, was that of a woman whose husband had just died from cholera, and this patient had not used the saline powders which he had ordered. The second unsuccessful case, was that of another woman in the same place, who was attended by Mr. Whitmore and another physician. In this case effervescing mixtures were given, made with *muriatic acid* and carbonate of soda; but this experiment did not succeed, and the woman died after a short illness.

The following case is one of those which occurred in Mr. Whitmore's private practice. It has already been published in the Medical Gazette, but as there are some facts in it which may not be without interest, I shall insert it here.

CHOLERA WITH ABORTION, BLACK DISCHARGE FROM THE UTERUS BECOMING FLORID UNDER THE SALINE TREATMENT.

' *To the Editor of the London Medical Gazette.*

' SIR,

' I AM induced thus publicly to communicate a recent case of cholera successfully treated by the *saline practice*, and under circumstances which may, perhaps, render its recital not altogether uninteresting to the profession.

' May 12th.—I was consulted in the afternoon, by Mrs. L., æt. 42, the mother of twelve healthy children, the eldest twenty-three and the youngest three years old. Supposes herself about three months advanced in utero gestation; complains of diarrhœa of two or three days' standing, with great prostration, and cramps in her lower extremities. Pulse infrequent, and feeble.

> ' Ordered ℞ Mist. Cretæ, f. ℥iv.; Tinct. Opii, ♏xl. capiat 4tam partem statim et repetatur post singulas dejectiones.

' In the evening her daughter came to say her mother had taken all the mixture without experiencing any benefit; indeed to her former symptoms were superadded vomiting of a violent character.

'Ordered the mixture to be repeated, with the addition of Tinct. Catechu, f. ʒs.

'13th, 6 A.M.—Mixture all used, but the patient is considerably worse. Has passed a very restless night; countenance much sunk; voice peculiar, and little more than a whisper; stools fæculent; a bilious fluid was also ejected from the stomach, and complains of a bitter taste in the mouth.

'Ordered Pulv. Cretæ, C. c. Opio, ʒj.; divide in Pulv. iv. to be taken in the same manner as the mixtures were directed.

'Noon.—The powders have all been taken, without the slightest improvement in any one symptom. Stools liquid, and now, for the first time, of a rice-water colour. The fluid which she vomits, however, is still bilious; tongue flabby, coated, and cold; pulse scarcely perceptible at the wrist; complains much of headach.

'℞ Liq. Opii Sedat. ʒj.; Ammon. Carb. ℈j.; Syrupi Aurantii, f. ʒvi.; Aquæ, ℥v. misce, capiat Coch. ii. secundâ quâque horâ cum Acid. Tart. gr. xv. in statu effervescentiæ.

'10 P.M.—No better. Some discharge from the uterus of *black blood*, with bearing-down sensations as if about to abort.

'14th, 7 A.M.—Miscarried in the night; appears to have been very correct in her calculation as to her period of gestation. Purging and vomiting not at all relieved; extremities cold; pulse gone, and appears to be fast approaching to a state of dissolution. Ordered a Seidlitz powder to be taken directly, and repeated at pleasure; also one of the following powders to be taken in twenty minutes after the Seidlitz, and repeated every hour:—

' ℞ Sodæ Carb. ʒss.; Sodæ Mur. ℈j.; Potassæ Oxymur. gr. vii. misce.

' Noon.—Vomiting has ceased; purging less frequent; pulse begins to be perceptible, but small slow, and tremulous; some return of heat on the surface. One Seidlitz powder has been taken; also four of the other powders; all of which have been retained. These were ordered to be continued.

' 10 P.M.—Nine of the above powders have now been used, and retained. Considerable re-action has taken place; voice and countenance much improved; pulse 80, and begins to be more full. *Lochia beginning to be florid in appearance*, and of the usual quantity. Only one dejection since my last visit. Powders ordered to be continued every hour. From this time she rapidly improved, and is now out of danger.

' It is worthy of remark, that the eldest daughter and the husband of this woman have also been attacked with diarrhœa and cramps in the extremities, for which Pulv. Cretæ C. c Opio was given, without affording the slightest relief. After persisting in their use for some time, and as the symptoms were evidently becoming worse, recourse was had to the above saline remedies, and speedy recovery was the almost immediate result.

'You will observe, sir, I was very tardy in putting these patients under the saline treatment; but I must beg to observe, that this did not arise from a conviction on my part that they were not cases of cholera, for, perhaps, a better marked case than the first was never witnessed; but I was anxious to try whether, while bile continued to pass into the duodenum, the diarrhœa and vomiting could not be arrested by any other means than the saline medicines, as recommended by Dr. Stevens, which I had seen used with so much success in the prison at Cold-Bath Fields. I think, however, you will agree with me, that I gave chalk and opium (the usual remedies) a very fair trial in the above cases. I am the more particular in pointing out this, as my belief *now* is that the non-purgative alkaline salts are, as Dr. S. asserts, more useful in relieving the sickness at the stomach, and checking the diarrhœa, than common astringent or absorbent medicines. I may also observe, that these cases show that the rice-water evacuations are not invariably present in the early stage of the Indian cholera; and from what I have seen of this malignant disease, though I believe that no treatment will be successful in every case of collapse, yet my thorough conviction is, that a much greater number of patients will be saved by the saline treatment than by any other practice that has yet been tried.

'I have been the more induced to communicate the above facts, as I observed that others (*even the Central Board of Health*) are still recommending the use of medicines *which have been long used in cholera, fairly tried, and found to be not only useless but actually injurious.*

'I am, Sir,
'Your obedient servant,
'HENRY WHITMORE.

'Cold-Bath Square, May, 21, 1832.

'I may add, that on last Sunday night, just before midnight, I was called on to attend another female, who resides in the same parish, and at no great distance from the above patients. This woman had been suffering for two or three days from vomiting and purging, but when I saw her, for the first time, she was in a *state of collapse*, and was also exceedingly emaciated from previous bad living. She was immediately put under the saline treatment; reaction soon came on, with general amendment in all the symptoms. Ever since she has continued to improve; the kidneys are again acting, and I have now great reason to hope that she may recover. She is, however, in a very low state, not merely from the effect of the disease, but also from previous ill health. She is suffering also from great mental anguish, for the loss of two children, one of whom had died on Saturday and the other on Sunday, the same day that she was herself attacked. I did not see either of these children, but both of them were reported as having died from confirmed cholera.'

'Thursday evening, 9 o'clock.'

The case last referred to in Mr. Whitmore's communication, is the patient on whom the saline treatment was first tried in the free hospital in Greville-street; she has since been dismissed cured, and is now in better health than she had been previous to the attack.

Soon after this case occurred, seven other patients were admitted into the same hospital, six of them were from Blue-court, Saffron-hill, and one from Holborn; two of the nurses who attended these patients were also attacked. These patients were attended by Mr. Whitmore, Mr. Marsden, and myself: one man, who was brought to the hospital in the last stage of collapse, died soon after admission. We also lost one of the nurses, a very stout woman, who was attacked most violently, on the 31st of May. She was put under the saline treatment, and on Monday, the 4th of June, was so far recovered from the state of collapse as to be considered out of danger. The same evening, about seven o'clock, she was attacked with a violent cramp in the stomach, which was probably followed by inflammation and organic disease in that organ. After this the irritation was so great, that even cold water could not be retained, and she died on the evening of the 6th. After her death it was ascertained, that almost immediately before she had been attacked with the cramp in the stomach, she had eaten *a whole lobster* (probably a bad one), which had been brought in to her clandestinely by one of her companions.

This woman, however, and the man, to whom I have referred, were the only two patients that were lost in the above hospital, from the period they commenced with the saline remedies. And I regret that in the first case which we lost we did not inject the saline fluid into the veins, nor try the effect of a *very large dose* of a saline solution given internally; for this I believe may be used, not only with impunity, but with great advantage in such cases.

The above cases occurred in an hospital into which the patients are generally brought very late, and often in the very worst forms of the disease, yet had it not been for the imprudent conduct of one of the nurses, out of ten malignant cases of cholera we should have saved nine: but, even as it was, out of these ten, eight patients were saved; and this is very different from the result which attended the common treatment in the same hospital, for under that, I believe, more than one half died.

I may here observe that the saline treatment was adopted in this hospital in direct opposition to the opinion of the medical gentlemen of that establishment. The remedies previously used had not been so successful as expected, consequently the saline treatment was adopted. Mr. Whitmore who lives, in the neighbourhood, was called in for the express purpose of seeing that the plan was properly pursued. This gentleman requested me to attend the cases along with him, which I did. Mr. Marsden, the surgeon to the establishment, also attended the cases with us. This gentleman was evidently opposed, at first, to the saline practice, and, perhaps, the more so, as the treatment had been in some measure forced upon him by some of the governors of the hospital. I had, however, the satisfaction afterwards of hearing Mr. Marsden make the following manly declaration in the presence of four other medical gentlemen. He stated firmly, "that no person could have thought less of the saline treatment than he did, until he had seen it tried; but after what he had witnessed in that establishment within the last fifteen days, that a man must be more than a sceptic who would refuse to admit the evidence of his own senses; and from what he had now witnessed, he was willing to admit that the saline practice was decidedly the most successful that he had seen tried."

The saline treatment has now been used, in that part of London, in about two hundred and twenty-six cases of cholera. Out of this number there have been about twenty deaths, and upwards of two hundred recoveries. It is true, however, that many of these were not cases of collapse; for this, where we saw them early, was generally prevented, by the immediate use of the saline treatment; but from what I have seen, my conviction is that, if these cases had been treated with the common remedies, the one-half of them would have been lost. Or, when we compare the result even in the most malignant cases, with the average mortality not only in London, but in other places, it will be found that the balance is greatly in favour of the saline treatment.

I might bring forward many additional facts on this subject, but I trust that I have already said quite enough to induce any impartial practitioner to give the saline treatment a fair trial in cholera; but as I have said before, my belief is, that there must be a very material change, both in the theory and the practice of medicine, before either this, or any other treatment, will be generally successful.

I should feel myself wanting in a proper sense of gratitude were I to omit this opportunity of expressing the deep obligation which I feel to Mr. Wakefield, whose conduct has been beyond all praise; and were it not for this gentleman, it is very probable that I should not have had an opportunity of trying the effect of this practice in the treatment of cholera. I had previously made several attempts towards giving it a trial, but in some of them I met with such discouragement, that I was compelled to give it up almost in despair.

I must also take this opportunity of returning my sincere thanks to the magistrates of Middlesex, for the liberal and kind manner in which they have been pleased publicly to express their approbation of the success of the treatment that was used in the prison which is under their care. I have also to thank them for their kindness to Mr. Crooke, who having seen the saline treatment extensively used in the West India fevers, was anxious to see it get a fair rial in cholera. When the first opportunity occurred he relinquished, for a time, his studies as a student; and though he had a firm belief that the disease was contagious, yet he cheerfully volunteered his services, and remained almost constantly in the prison, both day and night; and to his unwearied attention to the sick, but above all to the exemplary conduct of Mr. Chesterton, the governor of the prison, I believe we were indebted, in no small degree, for the success of our practice.

I have also to express my thanks to Mr. Whitmore, as well as to Mr. Marsden, Mr. Spencer, and other gentlemen, not only for their having given this practice a trial, but also for the manner in which they have expressed their conviction of its superiority to the methods of treatment in general use. I may add, that the candid and manly conduct of Mr. Wakefield, Mr. Marsden, and the whole of the gentlemen whom I have met in that quarter, is to me some consolation for the illiberal and unfounded attacks to which every individual must expose himself who ventures to appear before the public, even when he is actuated by the purest motives, or when that which he states is most strictly correct. But as I have formerly said, truth, whatever may assail it, will ultimately maintain its course, and those who attempt to impede its progress, though they may succeed for a time, yet they will at last find it as hopeless a task as it would be to prevent the mountain torrent from finding its way to the sea, into which it is ultimately to be received.

ON THE INJECTION OF SALINE SOLUTIONS IN CHOLERA.

THE injecting of medicated substances into a vein is neither a new nor a difficult experiment, and when we once find out the proper remedies to use for curing a disease, the mere act of throwing it into the vessels is exceedingly simple.

The operation of injecting a saline solution into the veins in cholera was, I believe, first proposed by Mr. Smart, in a letter which is dated Cranborne, November 14th, and published in the Medical Gazette for the 26th of November, 1831. Mr. Smart appears to have had but little faith in the common remedies; for he asks, 'What are venesection and calomel expected to effect? means, the best adapted, in my opinion, to finally extinguish the still flickering flame of life!'—and after alluding to the action of the salts on the blood, as stated in the paper which was read at the College of Physicians, he then adds, 'I would therefore propose, not only to try the injection of the above (namely, the saline) remedies into the veins, but also the transfusion of pure blood.' Mr. Smart also announces his intention of trying the saline injection, should circumstances render it necessary. I must observe, however, that in his hands it would not have been successful, for he appears to have but very confused ideas on this subject, and being misled, as it would appear, by the opinions of Dr. Clanny, he actually proposes to inject *carbonic acid*, at the same time, *into the veins*. He also recommends the use of brandy and opium, so that what he would have gained by the saline injections in the treatment of cholera, he would have lost by the carbonic acid, the opium, and the other improper remedies which he proposes to use.

On the 3d of December, that is a week after the above letter had been published, a similar proposal was made by the editor of the Medical Gazette, only this better informed individual did not recommend the injection of carbonic acid into the veins, for the purpose of giving an arterial colour to the black blood, which is so invariably met with in cases of cholera.

REMEDIES TRIED AT SUNDERLAND IN CHOLERA —OTHERS SUGGESTED.

'We mentioned last week, that a hogshead of brandy, already mixed with laudanum in due proportion, had been furnished by government for the use of the cholera patients at Sunderland. We regret to say, that little benefit has been derived as yet from this, the volatile oils, or any other method of treatment which has been tried; nor has anything occurred to throw any additional light, either on the nature or treatment of the disease. The inhalation of oxygen has been tried in several cases without any apparent advantage; the pulse, indeed, rose a little during the inhalation of the gas, but immediately on leaving it off, the sinking of the pulse was found to be greater than before its use. Some nitrous oxide was in preparation a few days ago, and ere this, we doubt not, has been tried. We must confess, however, that we entertain little hope from this class of remedies, because the lungs do not act upon that portion of oxygen which the air naturally contains, it having been long ago ascertained, by Dr. John Davy, in India, that only from one-fourth to one-third of the proper quantity of carbonic acid was found in the air expired by those labouring under cholera. A galvanic battery has been ordered to be sent to Sunderland, and the effects of this most powerful agent are to be ascertained. The experiments of the late Mr. Finlayson, at Ceylon, though too limited to warrant any general inference, were certainly such as to justify further trial: in one of his patients, who was " moribund," a galvanic current from a small battery was passed through the chest, the man immediately revived, and ultimately recovered. The same gentleman also states, that in two out of three cases, the functions of the lungs appeared to be restored by stimulating them with ammonia, volatilized so as to impregnate the atmosphere with its fumes.

' *We earnestly recommend a trial of injecting me-*

dicated solutions into the veins, particularly some of the neutral salts, as muriate of soda. No one who ever saw a leech disgorge its black blood upon salt, can fail to have observed its instant conversion into a brilliant scarlet. That the change of colour carries with it a corresponding change of character, we do not venture to assert; but it is at least worth ascertaining; and we think that the views of Dr. Stevens, as to the effect of salts on the blood, to which we have repeatedly called attention, ought to be put to the test as speedily as possible. It affords no mean claim to the investigation of these doctrines, that a man of Dr. Prout's high character should have declared the essay containing them, and which was published in this journal, to be one which, spite of some imperfections, seems to contain the germs of discoveries of the last importance to mankind.'

In the paper which was read at the College of Physicians, I had stated that the natural salts of the blood were not merely the cause of its red or arterial colour, but one chief cause of its stimulating power. There were some who denied that this was the fact, but did so without taking the trouble of performing a single experiment with a view of putting to the test whether the facts which I had stated were correct or not. There are others, however, who have gained great credit to themselves by having acted differently.

In a paper which was read at the Westminster Society, on the 3rd of December, and published in the Lancet of December 10th, 1831, Dr. O'Shaughnessy states that 'about *three months after* Dr. 'Stevens's researches and experiments were laid be'fore the public,' he had injected a solution of certain salts into the veins of inferior animals, and found that they not merely give a florid colour to the darkest blood, but enabled it to increase the excitement in the whole system, by adding to its power of stimulating the vascular organs. From these facts, Dr. O'Shaughnessy inferred, that as the cholera is more rapid in its progress than the yellow fever, that benefit might be derived, in certain cases, by injecting a solution of certain salts directly into the veins. The salts, however, which Dr. O'Shaughnessy recommended have not yet been injected into the veins, for at that period he recommended the oxygenated salts in preference to those that have been used, from a belief that they arterialized the blood by communicating oxygen; but this opinion he has since retracted with a degree of candour which does him great credit. There are, however, more important facts than one for which the profession are under great obligations to this gentleman, who has already commenced a brilliant career, and we have much to hope from his future labours.

About a week after Dr. O'Shaughnessy's paper had been read, another letter was published in the Medical Gazette. The following is an extract:—

‘EXPERIMENTS ON THE BLOOD.

‘*To the Editor of the London Medical Gazette.*

‘SIR,

‘The notices repeatedly inserted in the Medical Gazette of certain opinions advanced by Dr. Stevens, regarding the effects of some of the neutral salts on the blood, have naturally directed attention to this point, at a time when the failure of all ordinary methods of treatment in cholera has almost unavoidably led practitioners to inquire, whether there be yet any untried expedient which might by possibility be useful. About a month ago, having procured successive supplies of newly-drawn blood, I mixed it with different substances, obtaining the now well-known general result of rendering the fluid dark and thick by means of strong acids, and of a bright scarlet by means of the neutral salts. The transition from the Modena hue of venous to a vermilion, *resembling* that of arterial blood, is certainly a remarkable phenomenon; and those present agreed, that if anything was to be hoped for from effecting a similar change in urgent cases of cholera, it was most rationally to be attempted by direct injection into the veins—a process which Mr. Arnott undertook to perform, should circumstances occur to render the proceeding feasible,’ &c. &c.

The above letter was written by Dr. Macleod; and it is to be regretted that this proposal was not sooner put into practice; for if it had, there is little doubt that many individuals would have been saved who have since fallen victims to the disease. But, unfortunately, the benefit which has since been derived from the saline treatment was prevented for a time, partly by a circumstance already referred to; for the editor of the Medico-Chir. Review not only denied my statements in the public journals, but he candidly confesses that previously to their publication he had sent a copy of the Trinidad documents to Dr. O'S., for the express purpose, as he says, of preventing this gentleman from leaning to my side.

The proposal for injecting a saline fluid into the veins was first put in practice by Dr. Latta, of Leith. We are not informed at what date it was first tried; but the letter communicating the first information on this subject to the Central Board of Health is dated Leith, May 15th, 1832. The result of Dr. Latta's experiments is well known. It has also been since tried by others. In these also there has been some recoveries*, and several deaths; but I sincerely trust that the failures in future will be less frequent. The albumen which has been used is altogether unnecessary, for the blood in cholera has more consistence than the blood in health. The sharp-pointed silver instrument which is generally attached to Read's apparatus is too sharp at the point to be introduced with safety into a vein; and my conviction is, also, that the saline fluid which has been used is too large in quantity and not sufficiently strong; and this, in all probability, has been one chief cause of the many failures. We know it to be a fact, that when animals are killed in a state of exhaustion, or immediately after they have been taking very hard exercise*, a very large portion of salt is then required, on purpose to preserve them from the putrefactive process, and when the blood, as in cholera, is not merely deficient in saline matter, but is also in a diseased condition from other causes. It is then essentially necessary not only to supply the blood with the natural saline ingredients which it has lost, but to throw into the current a larger proportion than usual, for the purpose of enabling it to resist the destructive effects of the morbid poison.

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* The following is one of the many facts which may be brought forward to prove that the stronger salts are essentially necessary in the treatment of cholera. I saw one most interesting case of a fine little girl, about nine years of age, where the physicians who attended her had trusted the cure merely to the carbonate of soda combined with laudanum. Under this treatment, the stage of collapse came on so suddenly, and to such a degree, that it was judged necessary to inject a saline fluid into the veins. This was done, and the child recovered.

The injection, however, of saline fluids directly into the blood is as yet only in its infancy. I have no doubt that it will be the means of saving many lives; but it will seldom be required where the patients are seen early in the disease, and properly treated; consequently, where one individual will be preserved from cholera by this operation, a thousand will be saved by the internal use of the energetic non-purgative salts. There is one point of view, however, in which I consider Dr. Latta's experiments as possessing an intense value—and that is, inasmuch as they afford the most unequivocal evidence in favour of the opinions with respect to the effects of salts on the blood, which were first publicly communicated to the profession in the paper which was read at the College of Physicians.

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* See Wilson on the Blood.

When the cholera is left to itself, or even where it is treated in the beginning with improper remedies, it is almost incredible, to those who have not seen it, how rapidly it runs its course to a fatal termination; but it is equally incredible, when the disease is properly treated, how very soon it may often be arrested in its rapid progress. I have seen, in some of the very worst cases, where, when a few doses of the saline mixture could be retained in the system for a sufficient time to enter the circulation, the fatal symptoms were almost immediately arrested; and even when the collapse has commenced, after a short period, the pulse can be felt beginning to creep, animal heat begins to be evolved, and though the patients continue weak for a time, yet they gradually recover from the state of collapse.

In two of the most malignant cases which I have seen, there was no premonitory diarrhœa; and in one of them the bowels had not been open for three days previous to the attack: consequently, cholera is not merely an excessive diarrhœa, for neither the bowel complaint, the rice-water ejections, vomiting, nor cramps, are essential to this disease; and where these symptoms do exist, they are merely the effects of the poison—for they are merely accidental; but *a sudden coldness of the blood, and of course of the whole body, without any obvious cause, is, perhaps, the best characteristic symptom of this pestilential disease.*

In the first stage of cholera, the ejections are, in general, passed with great force; but as the disease advances, the intestines become cold, and frequently so torpid, that even hot saline enemata can be retained with great ease: consequently, in such cases, there is no necessity for plugging the rectum, as recommended by Dr. Clanny.

The rice-water ejections, which are generally passed so copiously in the first stage of cholera, are, like every other secretion in the body, derived entirely from the circulating current: and as the colouring matter of the blood is, perhaps, the only ingredient which is not drained off in this way, it naturally follows that a given quantity of black cholera blood must be more dense, and contain more colouring matter and less serum, than is met with in the red blood of a healthy person*. It is also a fact, that the arterial

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* The following are the proportions betwixt healthy blood and that in cholera, as given by Dr. Thomson. In the former,

Serum	55
Crassamentum	45
	100

Cholera Blood.

Serum	32·34
Crassamentum	67·66
	100·00

blood contains less air: there is also less carbonic acid in the venous circulation, as was ascertained many years ago by Dr. John Davy.

We have seen that, in the early stage of cholera, the contents of the stomach and intestines are ejected from the system with great force. This sudden and forcible contraction is probably caused by the poisoned or acrid quality of the secreted fluids, which are in this way removed from the body. The urine which is secreted at this period is also expelled with considerable force, even though it is not secreted in such quantity as to stimulate the bladder by distension; and this firm contraction of the bladder, even on the last drops of the acrid fluid which is expelled, is probably the chief cause why this organ is so firm and contracted after death.

We have seen that animal heat is generated and evolved in the extreme texture all over the body; consequently, everything that increases the action of the extreme vessels, adds to the quantity of animal heat. It is for this reason that I consider frictions with hot, dry flannel, but particularly the application of large sinapisms to various parts of the body, of great value, not only in cholera, but also in the malignant cases of the African typhus, and all other diseases where it is an object to increase the quantity of animal heat as much and as suddenly as possible.

During the convalescence the patients had generally a craving for salt food. This was remarkably the case with one boy, who had been in a state of complete collapse for nearly twenty-four hours. When the re-action commenced, the first use that he made of his speech was to beg for some salt mackerel. As this could not be obtained at the moment, he was allowed a part of a salt herring, which he ate with avidity, and wanted more.

I have written some further observations on cholera, which go far towards confirming the opinions on fever which I have already advanced in this work; but, as the present volume has already attained a size much larger than was originally intended, the observations which I intend to make on this subject will probably be published in a separate form.

APPENDIX.

THE following Notes, &c., having been omitted in their proper places, are inserted here.

The analysis inserted at *page* 3, relates only to the albumen. The fibrin differs somewhat in composition.

At *page* 4, for 'soluble in alcohol water,' *read* soluble in alcohol and water.

I have stated, at *page* 5, that a free alkali may be obtained from the serum after exposure to a certain heat. All that I mean by this is, that an alkali may be obtained in this way comparatively pure, for I am aware that an alkali cannot exist free from carbonic acid when heated in the air, and of course in contact with carbonaceous matter.

Note for page 30. I have more reasons than one for believing that the venous blood is neither forced on to the centre of the circulation by the atmospheric pressure, nor drawn into the lungs by means of a vacuum. My conviction is, that it is forced into the veins by the action of the extreme arteries, and possibly the large volume of oxygen in the pulmonary organs may have some effect in drawing the venous blood to itself, in consequence of the attraction which oxygen possesses for carbonic acid.

With respect to the experiment at *page* 73, I again repeat, that I do not mean to advocate the correctness of the reasoning which I used. It is probable that the carbonate of potass, which is formed during the ignition, may have had some effect in reddening the blood.

I have stated, at *page* 80, that carbonic acid is the heaviest of the gases: I mean by this, of those which belong to the atmosphere: for chlorine and some of the others are considerably heavier than carbonic acid.

I have stated, in *page* 81, that there is no chemical combination betwixt the water and the salts in a saline fluid; I am well aware, however, that this point is by no means settled. When we evaporate the water, the salts are obtained in their original form. Expansion and condensation are considered as leading characters of chemical affinity, and there is generally a condensation of volume when salts are dissolved to water.

It is stated, at *page* 101, in an extract from Dr. Mitchell's paper, that charcoal condenses gases, with a force that is sometimes equal to a power of forty atmospheres. The existence, however, of this species of condensation has been long known. Saussure, in 1811 or 1812, proved that the fibrous textures in many different forms of matter, such as charcoal, amianthus, &c. have the faculty of attracting and condensing large quantities of various gases. The real nature, however, of this phenomenon is at present as mysterious as the lifting up of carbonic acid by oxygen gas.

The experiment at *page* 107, was only introduced for the purpose of illustration. I am well aware that chemists account very differently for the change of colour in the wax from an almost black to a bright red hue.

Note for page 197. I have either mislaid or lost the notes which I had written at Bristol. The circumstances, however, are still so fresh in my memory, that there can scarely be any essential mistake in what I have stated. The only fact about which I have the slightest doubt in my own mind is the exact year in which it occurred: I think, however, it was in 1825. I may add, also, that when the vessel arrived at the quarantine ground (after a short passage from the West Indies) there were two sailors on board, who had been attacked with the African typhus so late in the voyage that they were actually ill when they arrived. These men were put into the quarantine hospital; one of them died, and the other, I believe, recovered. The cargo was taken out of the vessel, and sent up to the city in lighters. The brig was quarantined for thirty days, after which she was fumigated and whitewashed, and then permitted to proceed up the Delaware. The bag of infected clothes to which I refer, had either been concealed in some part of the vessel, or it had been smuggled aboard, about the time she was released from the quarantine. When she arrived at Bristol, this was sent by Captain Hutcheson to his father's house, and a few days afterwards the disease commenced.

As I have mentioned some circumstances relative to the fever at Philadelphia, in 1793, I may here observe, that the first case of the African typhus which then occurred, after an interval of many years, was that of the captain of the sloop Courier. This vessel arrived in the Delaware, after a short passage, with some French emigrants on board, from St. Domingo, where it is well known that the African typhus was then raging. It is probable that there had been some cases on board soon after she left St. Domingo; but this much is certain, that the captain was attacked a few days before her arrival; and when the vessel was going up the river, he was so ill that they were obliged to land him at Chester. This was on the evening of the 30th of July; and he died on the following morning, with the symptoms of the African typhus distinctly marked.

The Courier went up to the city during the night, and the emigrants were landed, probably, early on the 31st of July. They were allowed to carry their clothes, &c., with them; and the disease commenced in that very spot where these people landed. The first case in the city occurred on the 3rd of August—that is, about four days after the landing of these individuals; and this I believe is about the period that this poison requires to produce the disease. The case to which I refer occurred in the boarding-house of Richard Denie. This house is in Water-street, and near the wharf, where the Courier came to anchor, and was generally occupied with passengers and sailors. The first individual attacked was a woman named Parkinson, who was then living as a lodger in Denie's house. She was attacked on the 3d, and died on the 7th. Other cases soon made their appearance in the same neighbourhood; and after a short period, it began to creep in from this spot towards the centre of the city; and before the end of October, upwards of four thousand individuals fell victims to the disease.

Rush has justly observed, that 'in extensive epidemics no remedies can be generally useful, except those that are cheap, and capable of being used with but little attendance;' and though the mortality from the African typhus has been very great whenever this disease has occurred in the United States, yet I will venture to predict, that this fever will be less fatal, if it ever appears again in that country, if the physicians will only have the good sense not to do any harm with calomel, opium, antimony, or acids, but treat it with bleeding and Saratoga water in the first stage, and in the last with carbonate of soda and the other active alkaline salts, which do not act too much on the intestinal canal.

Note for page 289.—About the year 1740, an epidemic fever of the typhoid kind was very prevalent and very fatal at Berlin. One physician in that city became so famous for the success of his practice, that the Government was induced to purchase the prescription, which he had kept a secret. It turned out to be muriate of potass—then, as now, fallen into disuse, though at one time highly celebrated, under the title of the Digestive Salt of Sylvius, being accounted capable of promoting a favourable crisis in fevers, when freely administered in the last stage.

The above fact was communicated to me in a note which I have received from my friend Dr. Ure. The muriate of potass is certainly more pungent than any other salt, and I intend to give it a trial in cholera.

Note for page 471. Up to the present moment there has been in the prison at Cold Bath Fields about twenty-three new cases of cholera, and two additional deaths. The first was a case of relapse:—the patient had been admitted into the Cholera Infirmary on the 25th, with vomiting, purging, and severe cramps. He was immediately put under the saline treatment, and on the morning of the 27th he was considered so far convalescent as to be allowed to walk about the court; but the saline treatment had only been suspended one day, when this man was again attacked, and on the forenoon of the 29th he was re-admitted into the infirmary, with most severe cramps, particularly in the extremities; and in a few hours he was in a state of collapse to such a degree, that Mr. Crooke was induced to inject eight pints of a saline fluid into the veins. I saw him about one hour after the operation. He had *then* a good pulse, with a warm and a moist skin: he had also completely recovered his senses, and said that he felt much better. I had great hopes that we should have saved this man, but about ten o'clock he died suddenly. This was a case of relapse, and such cases have hitherto been invariably fatal; but still my belief is, that this man would have been saved if we had used only four pints of the saline fluid, in place of eight.

On the 29th, a woman, who was apparently perfectly well when she was locked up, about eleven o'clock, was attacked during the night; but as no communication of her illness had been made to the governor, she was found, about seven o'clock in the morning, in a state of complete collapse. Mr. Crooke then ordered some saline powders, and soon afterwards went for the injecting apparatus; but before he returned, the woman was dead. On the same day, one of the men in the cholera infirmary was so far gone in a state of collapse, that it was thought necessary to inject a saline fluid into the veins; but Mr. Crooke, in this case, could not succeed. He gave him, however, at one dose, about an ounce of muriate of soda, with ten grains of the chlorate of potass. Re-action almost immediately followed; and though this was also a case of relapse, yet he is now (sixteen hours afterwards) apparently nearly out of danger.

We have now had, during the present irruption in the prison at Cold Bath Fields, one hundred and five cases, and fifteen deaths; and there are at this moment in that establishment twenty-two patients who have recently recovered from a state of complete collapse. But though the proofs in favour of the saline treatment were as numerous as the sands on the sea-shore, still there are individuals who will deny its utility—and that too in a manner which can neither do credit to themselves, nor good to humanity. This is particularly the case with one of the leading members of the Central Board of Health, for I must say that the conduct of this gentleman has been anything but fair. He has been stating what is not correct with respect to the cases of cholera in Cold Bath Fields; and that too for no earthly reason, that I can see, except that he himself has done worse than nothing. It is very obvious, however, that, if I am right, this gentleman must be decidedly wrong; and therefore he appears anxious to impede the progress of any practice that is likely to be more successful than the unscientific and improper treatment which the Board have recommended*.

When patients are treated with the common remedies, such as recommended by the Board of Health, the disease is rapid in its progress, and few I believe have been known to recover from the state of collapse; but I may say with safety, that I have now seen at least fifty cases where the patients have been saved, even in the last stage of the disease, by the saline treatment, and where most assuredly all of them would have died had they been treated with cupping, or the application of red-hot irons to the nape of the neck—emetics, opium, and calomel given internally, with *muriate of soda and nitrous acid.* But I sincerely trust that the patients in future may be preserved from the destructive effects of such unscientific and improper treatment—which will not be the less fatal merely because it is the practice recommended by the Board of Health.

Note for page 459. The cholera infantum is very common in the United States of America, and under the usual treatment it is generally fatal. Dr. Dewees, however, who is one of the most scientific practitioners in Philadelphia, has been treating this disease with frequent injections of muriate of soda; and this has been found to be by far the most effectual remedy—not only for allaying the irritation at the stomach, but also for remedying the quality of the morbid secretions, and curing the disease. In short, it is said 'to act like a charm.'

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* I need scarcely say, that the gentleman to whom I allude is neither Sir William Pym nor Sir William Russell.

The above fact, which is stated in Dr. Dewees' Treatise on the Physical and Medical Treatment of Children, was pointed out to me lately by Dr. Rigby, a medical gentleman who is one of the lecturers on midwifery at St. Thomas's Hospital.

Note for page 486. There are various explanations of the manner in which certain salts produce their powerful effect, in preventing the putrefactive process. The following is the explanation of an eminent friend, who is at the very head of his profession as a practical chemist:—

'Water is the prime agent in the decomposition of all vegetable and animal substances, spontaneously liable to the putrefactive change. If these organic compounds be thoroughly desiccated and secluded from hygrometric moisture, they may be preserved for ages. How does the water act in such cases? It is by what Berthollet called disposing affinity, a force exemplified in the rapid action of phosphuret of lime on water, though neither phosphorus nor lime decompose that fluid. Thus also, the two elementary constituents of water readily exchange their liquid binary union for other combinations of a more complex nature, when invited by the various elements of organic nature and the force of elasticity. But if we present to the same water any substance which has a strong attraction for it as a liquid compound, and none for its *aërescent* constituents, then the quiescent affinity gains such an ascendency, that the divellent affinity though excited even by the contact of vegetable and animal products of a putrefactive kind, is no longer effective, or so feebly so, that the organic matter changes very slowly indeed. This appears to be the principle on which sugar, and various neutro-saline matters preserve or cure, as it is called, vegetables, butchers' meat, &c.'

I may observe, that during the present irruption of cholera in the prison at Cold-Bath Fields, one of the prisoners was attacked who had been labouring under dropsy for some time. He was getting rapidly into a state of collapse, when its progress was arrested by very large doses of the non-purgative salts. He is now out of danger, as far as relates to cholera, and the fluid in the abdomen has entirely disappeared.

THE END.

London: W. Clowes, Stamford-street.